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ALAN GREGG AND THE ROCKEFELLER FOUNDATION'S SUPPORT OF FRANZ ALEXANDER'S PSYCHOSOMATIC RESEARCH*

Theodore M. Brown

In recent years scholars have increasingly paid attention to the role of Rockefeller philanthropy in twentieth-century American medicine. With a growing depth of knowledge and subtlety of interpretation, they have studied the philanthropy's influence on public health, medical education, and clinical instruction.[1] They have also begun to investigate its role in the formation of molecular biology and in some areas of basic medical research.[2] As of yet, however, no scholar has devoted matching attention to Rockefeller influence in American clinical psychiatry, the neurosciences, psychoanalysis, or the closely related field of psychosomatic research.

This gap in recent scholarship is the more noteworthy because readily available secondary sources make it abundantly clear that, beginning in the 1920s, psychiatry and related fields were major focuses of Rockefeller philanthropic activity. Raymond B. Fosdick, for example, emphasized the importance of psychiatry and neurology in his history of the Rockefeller

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* Revised and expanded version of a paper presented at the fifty-eighth annual meeting of the American Association for the History of Medicine, Durham and Chapel Hill, North Carolina, 16 May 1985 I gratefully acknowledge research grants from the Rockefeller Archive Center, the advice and assistance of Arthur Viseltear and John Parascandola regarding the Alan Gregg Papers at the National Library of Medicine, the helpful comments of the *Bulletin*'s anonymous reviewers, and the suggestions of several friends who took the time to read an earlier draft. Mario Biagioli, John Burnham, Harold Dorn, Ian Dowbiggin, Stephen Kunitz, Corinne Sutter, and Mary Young

[1] A sampling of recent work includes Howard S Berliner, *A System of Scientific Medicine Philanthropic Foundations in the Flexner Era* (New York. Tavistock, 1985), E Richard Brown, *Rockefeller Medicine Men Medicine and Capitalism in America* (Berkeley and Los Angeles University of California Press, 1979), Ellen C Cangi, "Abraham Flexner's philanthropy the full-time system in the department of surgery at the University of Cincinnati College of Medicine, 1910–1930," *Bull Hist Med*, 1982, *56* 160–74, John Ettling, *The Germ of Laziness Rockefeller Philanthropy and Public Health in the New South* (Cambridge, Massachusetts Harvard University Press, 1981), Elizabeth Fee, "Competition for the first school of hygiene and public health," *Bull Hist Med*, 1983, *57* 339–63, Daniel M. Fox, "Abraham Flexner's unpublished report foundations and medical education, 1909–1928," *Bull Hist Med*, 1980, *54* 475–96, Kenneth M Ludmerer, *Learning to Heal The Development of American Medical Education* (New York. Basic Books, 1985), esp chap 10, "The Era of Medical Philanthropy"

[2] See, for example, Pnina Abir-Am, "The discourse of physical power and biological knowledge in the 1930s a reappraisal of the Rockefeller Foundation's 'policy' in molecular biology," *Soc Stud Sci*, 1982, *12* 341–82, Robert E Kohler, "The management of science the experience of Warren Weaver and the Rockefeller Foundation programme in molecular biology," *Minerva*, 1976, *14* 279–306, Edward J Yoxen, "Life as a Productive Force Capitalizing upon Research in Molecular Biology," in *Science, Technology, and the Labour Process Marxist Studies*, ed. L Levidow and Robert M. Young, 2 vols (London CSE Books, 1981), 1 66–122

Foundation, which, though published in 1952, is still the standard account.[3] Likewise, Wilder Penfield elaborated upon the Foundation's role in psychiatry, the neurosciences, and psychoanalysis in his widely available biography of Alan Gregg, director of the Foundation's Division of Medical Sciences from 1931 to 1951.[4] Moreover, Robert Morison noted that during Gregg's tenure the Foundation appropriated sixteen million dollars to these areas, and that this sum represented about two-thirds of its total expenditure in the medical sciences during those two critical decades.[5] These important observations in secondary literature written in the fifties and sixties seem to have largely dropped from the view of contemporary historians of medicine.[6]

This essay is an attempt to eliminate the blind spot in recent scholarship by focusing on a revealing example of Rockefeller Foundation support: the funding of the Chicago Institute for Psychoanalysis from 1935 to 1943 and Gregg's effort during that funding period to guide the Institute's research energies into the psychosomatic field. The Chicago Institute was one of the first psychoanalytical training institutes established in the United States.[7] It was directed from its founding in 1932 until 1956 by Dr. Franz Alexander, an emigré analyst from Berlin. In 1935 the Rockefeller Foundation awarded the Institute a three-year grant of $100,000, and in 1938 it added a five-year grant of $120,000. According to statements in his *Annual Reports*, Gregg expected the Chicago Institute to "provide psychoanalytic training of teachers and investigators in psychiatry" and also to support "clinical research, particularly in the correlation of somatic and psychological disturbances."[8] Funding of Alexander's operation was part of Gregg's overall plan for psychiatry and clinical medicine and, because of its projected role, is especially interesting to study.

Rockefeller funding of the Chicago Institute likewise merits study because it provides an excellent opportunity to explore Alan Gregg's goals and his methods of work. By the time he formally recommended support for this project within the Foundation, Gregg had already worked out in his own mind an overall plan for what in official documents he called "psychobiology," an interdisciplinary amalgam of the clinical, biochemical, biological, and psychodynamic aspects of psychiatry and neurology.[9] He had also closely studied Alexander and his Institute, investigated the citywide condition of medicine, psychiatry, and neurology in Chicago, and become familiar with potentially pivotal individuals and institutions in the region. Only after completing this careful preliminary study and assuring himself that his own vision of an apposite arrangement for Chicago had at least a chance of realization did Gregg allow the Rockefeller grant to Alexander to move through the Foundation's formal approval process.

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[3] Raymond B. Fosdick, *The Story of the Rockefeller Foundation* (New York: Harper & Brothers, 1952), chap. 10, "Medical Research and Psychiatry." Fosdick served as Foundation president from 1936 to 1948.

[4] Wilder Penfield, *The Difficult Art of Giving. The Epic of Alan Gregg* (Boston: Little, Brown, 1967).

[5] Robert Morison, "The Alan Gregg lecture 1964. some illnesses of mental health," *J. Med. Educ.*, 1964, *39*: 985–86.

[6] John C. Burnham, Gerald Grob, and Nathan G. Hale seem to be the only historians of medicine who have recently noticed the Rockefeller Foundation's support of psychiatry and psychoanalysis during the interwar period. They pay little overall attention to the Rockefeller role, however, and their attitude is generally skeptical or suspicious of the Foundation's intentions. See Burnham, "The Influence of Psychoanalysis upon American Culture," in *American Psychoanalysis: Origins and Development*, ed. Jacques Quen and Eric T. Carlson (New York: Brunner/Mazel, 1978), p. 70, n. 17, Grob, *Mental Illness and American Society, 1875–1940* (Princeton, New Jersey: Princeton University Press, 1983), pp. 158–59, 163–64, 266–67, 271, 309–10, 312–13, Hale, "From Berggasse XIX to Central Park West. the Americanization of psychoanalysis, 1919–1940," *J. Hist. Behav. Sci.*, 1978, *14*: 305.

[7] George H. Pollock, "The Chicago Institute for Psychoanalysis from 1932 to the Present," in Quen and Carlson, eds., *American Psychoanalysis*, pp. 109–26.

[8] *Rockefeller Foundation Annual Report for 1938* (New York, 1938), pp. 171–72.

Thus, studying Gregg's support of the Chicago Institute for Psychoanalysis can reveal a great deal about how he and the Rockefeller Foundation operated during the twenties, thirties, and early forties. To pursue this study, we will have to consider Alan Gregg in his intellectual, professional, and administrative development, see Franz Alexander against the background of the field of psychosomatics and the psychoanalytical movement, and examine the interaction between them in the context of the personalities, institutional politics, and tactics of opportunity that were part and parcel of Rockefeller philanthropic involvement with American medicine during the interwar years.

Let us first consider Alan Gregg. Born in 1890 as the seventh child of a Harvard-educated Congregational minister, he followed family tradition by attending Harvard College.[10] Here he at first unenthusiastically pursued an eclectic course of study, but he soon became interested in psychology and as a sophomore wrestled with the theories of William James. In mid-September 1909, an extraordinary event occurred that was to leave a mark on Gregg's later career. Through Harvard contacts, he was invited to the Adirondack vacation retreat owned in part by Dr. James Jackson Putnam, the famous Harvard neurologist and early American supporter of Freud. Just as Gregg was struggling to work out his own embryonic psychological theories, three foreign guests arrived at "camp": Sigmund Freud, Carl Jung, and Sandor Ferenczi. The impact was enormous. As Gregg later recalled, "I asked Dr. Putnam who they were, and he out of the sweetness of his heart gave a college sophomore an explanation of Freudian theory. I saw Freud. He was in the flesh."[11]

The experience stimulated Gregg to think about Freudian ideas.[12] He soon tried to read Freud's works and to struggle with undiluted Freudian concepts. In 1910, while still a Harvard undergraduate, he began a "Dream Book," a notebook in which he outlined the essence of Freud's theory of dream interpretation and carefully recorded and analyzed his own dreams.[13]

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[9] *Rockefeller Foundation Annual Report for 1934* (New York, 1934), pp 77–78

[10] The basic features of Gregg's life and career are derived from Penfield, *Difficult Art of Giving*, and "The Reminiscences of Alan Gregg," a partial oral history memoir interrupted by Gregg's death in 1957 and prepared by Dr. Saul Benison for the Columbia Oral History Research Office A typescript of the "Reminiscences" is on deposit at Columbia University and is available on microfiche Unless otherwise and specifically noted, biographical details are drawn from these two sources

[11] Gregg, "Reminiscences," p 28

[12] See Gregg's personal diaries for 1909 and 1910, especially the entries for 21 and 28 Sept. and 2 Oct. 1909, and 25 Apr, 29 May, 13 July, 29 Oct. 1910, in Box 4 of the Alan Gregg Papers at the National Library of Medicine, History of Medicine Division, Manuscript Collection 190 The Gregg Papers at the National Library of Medicine are contained in thirty boxes. (Hereafter referred to as Gregg, NLM, with box number.)

Gregg entered Harvard Medical School in 1912 convinced that he could incorporate psychoanalysis within a medical career. In December of his first year he took time out to read, in German, Freud's *Three Essays on Sexuality*.[14] He also took an elective course with Elmer Southard of the Boston Psychopathic Hospital during his clinical years. But the unrelenting pressures of the curriculum and the unmistakably laboratory-oriented values of his new environment began to affect him He became interested in science, for which he had felt no enthusiasm as an undergraduate (when he identified with the "literary crowd"). At Harvard Medical School, Gregg was invited to do research, and he responded quite positively. His most prized opportunity was the chance to work with Alexander Forbes, one of Walter B. Cannon's assistants in physiology. The result was an important paper that Gregg produced jointly with Forbes which demonstrated the identity of nerve impulses, whether originating within the brain and spinal cord or elicited by artificial stimulation of peripheral nerves.[15] The Forbes-Gregg paper was published in the prestigious *American Journal of Physiology*.[16]

After medical school, Gregg won a place on the highly competitive medical service of the Massachusetts General Hospital. Here he was exposed to the exhausting demands of the internship year and the fresh excitement of physiologically and biochemically-based scientific medicine recently brought to MGH by David Edsall, Harvard's Jackson Professor of Medicine since 1912.[17] Gregg also learned about teamwork and about chemical diagnosis.

At this point Gregg was uncertain about the direction of his career. Since he did not feel comfortable opting for private practice, he joined several other young physicians from MGH to serve with the Harvard Medical Unit treating war casualties in France. Between periods of intense activity he found himself with time on his hands. He started a private journal in November 1918, and in his early entries he recurrently notes his search for a motivating and energizing direction for his career.[18] Another theme is a certain skepticism regarding the adequacy of science as the sole basis for medical practice.[19]

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[13] Gregg, NLM, B 5

[14] Personal diary entry for 26 Dec 1912, Gregg, NLM, B 4

[15] On the significance of this paper, see John Fulton, *Selected Readings in the History of Physiology* (Springfield, Illinois Charles C Thomas, 1930), p 220

[16] "Electrical studies in mammalian reflexes," *Amer J Physiol*, 1915, *37* 118–76

[17] For Edsall's orientation and his impact on Harvard, see Joseph C Aub and Ruth G Hapgood, *Pioneer in Modern Medicine David Linn Edsall of Harvard* (Cambridge, Massachusetts Harvard Medical Alumni Association, 1970), esp chaps 4 and 12

[18] Penfield, *Difficult Art of Giving*, describes the journal on pp 89–97 The journal can be found in Box 5 of the Gregg Papers at the National Library of Medicine

Upon his return to civilian life Gregg felt lucky to land a job working for the Rockefeller Foundation on hookworm disease in Brazil. He left for South America in March 1919 and soon found himself waging "guerilla warfare on disease." Yet, after a while, he began longing for something more systematic, regularly gratifying, and intellectually challenging. He felt a restlessness that he managed partially to restrain by reading, fashioning entries for his private journal, and maintaining an extensive correspondence. One of his correspondents was Eleanor Barrows, whom he later married. With Eleanor he shared his continuing if somewhat tempered enthusiasm for psychoanalysis: "Write me the name of any Freudian journal which is worth taking. I do not know of any and I want to keep track of that line of ideas; sometimes I regret I didn't plunge into it long ago when I had the inside track."[20]

Gregg's career took another turn in 1922, when Dr. Richard Pearce offered him a job as his assistant in the Rockefeller Foundation's Division of Medical Education. The division had been created in December 1919 and was still in a formative stage.[21] Before joining the Foundation, Pearce had served at the medical school of the University of Pennsylvania, first as professor of pathology and then as chairman of the newly created Department of Research Medicine. He had trained under Simon Flexner (before Flexner was appointed director of the Rockefeller Institute for Medical Research in 1902) and had been allied with David Edsall in the struggles for "scientific medicine" at Pennsylvania (before Edsall left for Harvard in 1912).[22] When Gregg joined Pearce in 1922, Gregg's chief still looked up in awe to Simon Flexner (now a leading trustee of the Rockefeller Foundation) and tried to shape the division according to Flexner's expectations.

When Gregg first shifted to the Division of Medical Education, Pearce was principally concerned with the strategic funding of key medical schools around the world, foreign medical fellowships, and emergency aid to depressed and devastated post-war Europe. Gregg gravitated to the European work, and in 1924 arranged for a transfer and promotion he became associate director of Medical Education in charge of European operations.

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[19] A telling observation recorded by Gregg and quoted by Penfield (*Difficult Art of Giving*, p 95), is the following "The scientific training permits of reliability and accuracy, not adequacy Thus from these it follows that medicine may be both a science and an art."

[20] A. Gregg to E Barrows, 14 Nov 1919, Gregg, NLM, B 5 This letter is partially quoted by Penfield, *Difficult Art of Giving*, pp 106–7

[21] Alan Gregg, "The work of the Rockefeller Foundation in medical education and the medical sciences, 1920 to 1929 inclusive, under the direction of Richard Mills Pearce, Jr , M.D ," *Rockefeller Found Quart Bull*, 1931, *5* 358–75

[22] A. McGehee Harvey, *Science at the Bedside Clinical Research in American Medicine, 1905–1945* (Baltimore Johns Hopkins University Press, 1981), pp 144–47, 216 For some of the connections between Pearce and Edsall, see also Aub and Hapgood, *Pioneer in Modern Medicine*, pp 41, 94, 101, 281

After mastering the routine in the Foundation's Paris office, Gregg immersed himself in a major project: an "absurdly thorough" on-site survey of Italian medical schools which would occupy the majority of his time through April 1925.[23]

Gregg's survey was interesting in a couple of ways. Its emphases clearly reflected Pearce's concerns and the priorities he maintained in his division. As Simon Flexner's protégé, Pearce stressed preclinical science and rigorously experimental medical research. As Pearce's loyal associate, Gregg stressed these things, too. His report paid particular attention to instruction in laboratory science and concluded that Italian medical schools put "undue emphasis" on botany and anatomy at the expense of chemistry, physics, biochemistry, physiology, and bacteriology. Gregg urged that key individuals be selected from among the Italian preclinical faculties and that these men be entrusted with annual grants from the Rockefeller Foundation which would be used to train young investigators in a few select fields. Among the fields Gregg particularly recommended were physiology, general pathology, and pharmacology.[24] It is also noteworthy that almost every Italian medical school Gregg visited offered instruction in psychiatry or in "nervous and mental diseases," yet Gregg gave these activities scant notice and accorded whatever research may have been underway little attention. Simon Flexner was the "mailed fist in the glove" of Pearce's division, and Flexner was notoriously skeptical about psychiatry.[25]

By late 1925, however, the center of gravity of the Division of Medical Education had slightly but noticeably begun to shift. Broad changes in Rockefeller philanthropic activity were underway, and the ramifications of these changes were felt in all divisions of the Foundation. The principal source of the rumbles within was the Laura Spelman Rockefeller Memorial, a philanthropic entity rededicated in 1923 to what soon turned into massive support for the social and behavioral sciences[26] Rockefeller Foundation President George Vincent noted in his 1925 *Annual Report* that the Foundation, responding to recent changes within the Rockefeller philanthropic family, would begin to direct some of its energy and resources to studying "at least the borders of the fields of biology and psychology, as these have a bearing on medicine and public health."[27]

Pearce knew that his Division of Medical Education would have to cooperate. He announced in December 1925 that one of its "future projects" would be support for the Institute of Psychiatry in Munich under the direction of Emil Kraepelin, the world's leading psychiatrist.[28] Throughout his long career, Kraepelin had been committed to a vision of psychiatry in which the researcher's task was "to investigate the structure and function of psychopathological phenomena with the methods of natural science, and to combat mental diseases with the results."[29] Support to Kraepelin could therefore bring Simon Flexner on board at a time when, in George Vincent's words, "psychiatry was coming to the front as a most important subject which has been too much ignored," and in this fashion the Division of Medical Education would keep in step with the rest of the Rockefeller philanthropic family.[30]

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[23] Alan Gregg, "Medical Education in Italy," Rockefeller Foundation Archives, Record Group 1 1, Series 751, Box 3 (Hereafter referred to as RFA, with record group, series, box, and file designations where appropriate) The 339-page typescript is dated Oct. 1924–Apr 1925

[24] *Ibid*, pp 317–18

[25] The quoted phrase is from Gregg's "Reminiscences," p 218. Clear evidence of Flexner's skepticism about psychiatry is contained in a letter from Starr Murphy to George Vincent dated 8 Mar 1921 (RFA, RG 3, S 906, B 2, F 17)

[26] See Martin Bulmer and Joan Bulmer, "Philanthropy and social science in the 1920s Beardsley Ruml and the Laura Spelman Rockefeller Memorial, 1922–1929," *Minerva*, 1981, *19* 347–407.

[27] *Rockefeller Foundation Annual Report for 1925* (New York, 1925), p 51

Alan Gregg went along unhesitantly with the division's partial shift toward psychiatry He also ratified Pearce's implicit endorsement of the predominantly biological orientation of the Kraepelin Institute. Indeed, when Gregg first became directly involved with the project, in November 1925, one of his tasks during on-site inspection was to explore the status of the various biological disciplines. It is also apparent from Gregg's reports, however, that his curiosity and imagination again began to travel beyond the confines of the strictly "natural science" approach and toward psychoanalysis. On 11 November he recorded portions of his conversations with Kraepelin: "I questioned K. offhand on any shift that may have taken place in his interests in psychiatry in the last 10 years, and he replied that the development of serology had greatly changed his attitude.... He mentioned nothing besides serology—he is openly intolerant of Freud and Jung."[31] Gregg did not let the matter rest with Kraepelin's open intolerance. In his official report he questioned the close-mindedness he had encountered in Munich.[32] It seems clear that the new interest in psychiatry at the highest levels of the Rockefeller Foundation gave Gregg license to revive his own longstanding but until recently dormant fascination with psychiatry and psychoanalysis.

Involvement with the Kraepelin Institute had another effect on Gregg: it motivated him to conceive an ambitious plan for the continuing support of psychiatry by the Division of Medical Education. He communicated his idea to Pearce in December 1925, and within the month Pearce approved the plan in principle.[33] During the next several years Gregg believed he had the authority to examine the pieces of a possible program in psychiatry when-ever and wherever he found the time and opportunity. But both because it was still the assumed intellectual style of the Division of Medical Education and because by this time he had almost certainly come to believe in the orientation himself, Gregg approached psychiatry from a predominantly biological perspective.

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[28] "Report of Committee on Interboard Conference on Mental Hygiene, Psychology, Psychiatry, etc 16 December 1925," RFA, RG 3, S 906, B 2, F 17

[29] This is from Stanley Cobb's summary and translation of remarks made during ceremonies at the Kraepelin Institute in June 1927 (RFA, RG 1 1, S 717A, B 9, F 54)

[30] The quoted phrase is from President George Vincent's official diary for 2 July 1925, excerpt in RFA, RG 1.1, S 717A, B 9, F 54

[31] Gregg office diary entry for 11 Nov 1925, RFA. (Hereafter office diary)

[32] Alan Gregg, "Present Status—Scope of Work," 1925, RFA, RG 1 1, S. 717A, B 9, F 54

[33] Cf Gregg's office diary entry for 4 Dec 1925 and Pearce's reply, R. Pearce to A. Gregg, 28 Dec. 1925, RFA, RG 3, S 906, B 1, F 3

The tiles Gregg scrutinized with greatest attention for the mosaic he hoped to fashion in psychiatry are most apparent in diary entries for his visits with Eugen Bleuler in Zurich and Oskar Vogt in Berlin. When he visited Bleuler in January 1926, Gregg noted critically that he found no evidence in Zurich that "the essential medical sciences [were] being brought to bear on psychiatric problems."[34] By contrast, when he visited Vogt in November 1928, he gushed enthusiastically about the unusual "comprehensiveness" of approach he discovered, which he thought "both admirable and impressive."[35] Vogt had earlier been interested in the implications of hypnosis, and now he and his associates combined biological research on the cellular architecture of the brain, experimental genetics, and human neuropathology with physiological studies (including experiments in Pavlovian conditioning), clinical investigation, and biochemical research. Since Vogt had access to four thousand hospitalized psychiatric patients, he was able to construct in Berlin the ideal combination of clinical psychiatry and the "essential medical sciences" which Gregg had failed to discover in Zurich.

Gregg also cultivated a renewed interest in psychoanalysis at this time. In part, no doubt, he simply allowed his old fascination to come once more to the fore now that he was officially permitted to explore the field of psychiatry. In at least equal measure, however, he shrewdly allowed himself to follow relatively passively the interests of certain figures in the social, cultural, and scientific elite. Many such individuals had become intrigued with psychoanalysis by the late twenties, and by selectively reporting in his official diary portions of conversations with Americans in various European cities, Gregg could communicate a considerable and widespread enthusiasm to his still skeptical chief, Pearce.[36] Thus, "*3 November, 1928* [Berlin]: . . . [Dr. Joseph Aub, on leave from Harvard Medical School] said that Stanley Cobb [the Harvard neurologist, currently in Europe] has ceased stuttering after being psychoanalyzed by Seif in Vienna."[37] And when Gregg found the following statement in a letter from Cobb passing along information about Vogt, he doubtlessly felt confirmed in his belief that psychoanalysis ought to be one tile in the psychiatric mosaic: "What I feel is needed in psychiatry just now is cooperation between men who know some organic things and those who are philosophers (albeit practical ones) doing psychoanalysis."[38] According to Cobb (and Gregg), psychoanalysis should be granted a place in a new psychiatry, a yet to be created interdisciplinary field that would be dominated by biology and the spirit of investigation but open to the "stimulus" of Freudian and Jungian ideas.

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[34] Office diary entry for 19 Jan 1926

[35] *Ibid*, 5 Nov 1928

[36] For the general attractiveness of psychoanalysis to segments of the cultural elite in the 1920s, see John C Burnham, "From avant-garde to specialism psychoanalysis in America," *J Hist Behav Sci*, 1979, *15* 129

[37] Office diary entry for 3 Nov 1928

While Gregg was busy in Europe, developments were underway at Rockefeller headquarters in New York which would soon have an important impact on his still embryonic program in psychiatry. These centered on the reorganization of the Rockefeller Foundation itself and of the Rockefeller philanthropic boards in general.[39] Administrative restructuring was part of a larger reorientation: the shift in Rockefeller policy from support of education and application to support for the generation of new knowledge through research. Reorganization was formally completed in 1928 and immediately created both anxieties and opportunities for Gregg. His principal anxiety was that the Division of Medical Education—soon to be renamed the Division of Medical Sciences—would be either overwhelmed or absorbed by other divisions of the Foundation.[40] But along with that anxiety came the opportunity to push for a bold agenda in the medical sciences which would include a broadly defined program in psychiatry.

The first glimmerings of that new program were apparent at the close of the twenties. A November 1928 report of a special committee chaired by the Harvard dean and Rockefeller Foundation trustee David Edsall urged Pearce's division to work with existing medical school departments of psychiatry and neurology in an "attack upon psychiatric problems through much closer relations with the fundamental sciences and by use of measurable and precise methods."[41] At a high-level staff conference on 13 January 1930 Pearce offered a "minimum estimate" of five million dollars for this program over the next ten years.[42] The most concrete action taken by the division at the end of the 1920s, however, was one in which Gregg played a major role and in which he had a substantial personal stake: support for Vogt's "comprehensively" conceived Institute for Brain Research near Berlin.[43] On 22 May 1929 the trustees voted aid to Vogt just as, three years earlier, they had pledged it to Kraepelin; like the earlier grant to Munich, the total appropriation to Berlin was comfortably over three hundred thousand dollars.

Shortly thereafter, Gregg had to contend with fundamental changes in his professional life deriving from the sudden and unexpected death of Richard Pearce in February 1930. In October, the trustees offered Gregg the option of succeeding Pearce and he accepted, effective January 1931. In the latter part of 1930 Gregg had his first unrefracted view of the problems awaiting him. On a trip to the United States he discovered to his dismay that he had to justify the programs in medical science, including the budding one in psychiatry, practically de novo. One reason was the skepticism of the poorly informed new Foundation president, former physicist Max Mason.[44] A second was the Division of Medical Science's long-feared competition with the Division of Natural Sciences, which Mason had directed from 1928 until his elevation to the presidency in 1930.[45] Third, Gregg had to deal with a surprising special memorandum submitted by David Edsall in which Edsall characterized psychiatry as a field "dominated by elusive and inexact methods of study and speculative thought."[46] Edsall urged slow and cautious investigation via the exclusive pursuit of "real scientific studies" in "forcible laboratories or institutes, prepared to study such matters related to psychiatry as are subject to attack by the methods of more or less exact science." Psychoanalysis, on the other hand, should be bypassed entirely because it could not be "at present accurately studied from a scientific viewpoint." Indeed, psychoanalysis was dangerous because it had already shown a strong emotional appeal to able young men, highly trained in science, who entered psychiatry but "through the fascination of psychoanalysis gave up their scientific training practically entirely." Gregg's multidisciplinary program for psychiatry, dominated by the biological sciences but preserving a definite place for psychoanalysis, was clearly in jeopardy.

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[38] S Cobb to A. Gregg, 17 June 1928, RFA, RG 1 1, S 717A, B 10

[39] See Fosdick, *Story of the Rockefeller Foundation,* chap 11, and, for more detail, Robert E Kohler, "A policy for the advancement of science the Rockefeller Foundation, 1924–1929," *Minerva,* 1978, *16* 480–515

[40] For a clear indication of these anxieties within the division, see R. Pearce to A. Gregg, 10 Feb 1930, Gregg, NLM, B 24

[41] "Summary of Report of the Special Committee on the Division of Medical Education," RFA, RG 3, S 906, B 1, F 17, p 21

[42] RFA, RG 3, S 906, B 1, F 4

[43] For evidence of Gregg's deep involvement and personal investment, see Pearce's letter to Gregg, 30 Jan. 1929, Gregg, NLM, B 24

By this stage of his career, however, Gregg had become a seasoned Foundation officer who knew how to operate effectively in difficult organizational circumstances. Thus, in 1931 and 1932 he moved adroitly on several different fronts, relying in large measure on his personal skills and political acumen as he maneuvered. He made himself available to leading figures in the Rockefeller orbit for special projects at the same time that he enlarged his outside reputation.[47] Gregg also consulted with leaders of American psychiatry such as Adolf Meyer and William Alanson White, using reports of their concerns as devices for bringing the plight of American psychiatry to the urgent attention of Mason and key trustees.[48] With modest supplementary allocations to the Munich Institute for Psychiatric Research in 1931 and the Berlin Institute for Brain Research in 1932, he deftly reminded the Foundation of its commitments made in the late twenties.[49] He also cleverly made selective fellowship awards, which he promoted as a safe, practical, and cost-effective administrative option for the Division of Medical Sciences.[50] Perhaps most important, Gregg carefully selected a large project of the strictest possible "real" scientific focus—the Montreal Neurological Institute headed by neurosurgeon Wilder Penfield—as his major early effort as chief of Medical Sciences. After exploring this project carefully late in 1931, Gregg early in 1932 proposed supporting the Neurological Institute with a composite appropriation of direct grants and endowment totalling $1,282,652.[51] By August 1932, when he left for three and a half months to investigate Rockefeller-supported medical education in China at the personal behest of the highest authorities in the philanthropy, Gregg not only had gotten endowment for Penfield's Institute through the trustees and submitted a memorandum calling for a focused program in "human personality" but also had again openly discussed the place of psychoanalysis in psychiatry with Harvard's Stanley Cobb.[52]

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[44] Mason's skepticism and Gregg's testiness show through in the minutes of a staff conference held on 7 Oct. 1930 (RFA, RG 3, S 906, B 1, F 4, p 5)

[45] Evidence for Gregg's state of mind in late 1930 and early 1931 regarding internal Foundation politics is contained in a personal diary entry of 4 Feb 1931 (Gregg, NLM, B 5) "Took luncheon with A[braham] F[lexner] He offered to talk to Fosdick anytime I needed it. I told him I would never request that. I asked him if he knew where I stood with the Trustees or other officers and later explained that I had been unsettled by MM's raising the question as to whether there should be a continued action of the R.F boards in association with the development of medicine And that he had told Fosdick I was the best man they could get. No remarks on the others' attitude."

[46] Edsall, "Mental Health Memorandum Regarding Possible Psychiatric Developments," 3 Oct. 1930, RFA, RG 3, S. 906, F 19

[47] See Gregg's shrewd reflections in his personal diary for 9 Apr 1931, Gregg, NLM, B 5

Gregg's clever campaign paid off handsomely in April 1933, when the trustees agreed to allow the Medical Sciences Division to devote "special attention to the sciences underlying psychiatry" because, broadly conceived, psychiatry was "the most backward, the most needed, and the most probably fruitful field in medicine."[53] Given the dire financial circumstances of the Depression, existing activities of the division would be continued with "varying degrees of reduction or retrenchment," yet Gregg would be allowed to explore new psychiatric projects for possible Medical Sciences sponsorship. Gregg was expected to cooperate closely with Warren Weaver, Mason's protégé and the new director of the Natural Sciences Division, in a joint program on "the modern science of man."[54] But Gregg, though formally linked with Weaver, still had sufficient independent operating room to push through grants of $80,000 and $58,000 for neurophysiological research at the Johns Hopkins University and at Washington University in St

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[48] See especially Gregg's office diary entries for 19 and 24 Feb 1931 and 30 June 1932

[49] See, respectively, *Rockefeller Foundation Annual Report for 1931* (New York, 1931), pp 166–69 and *Rockefeller Foundation Annual Report for 1932* (New York, 1932), pp 213–14

[50] On the importance of fellowships in general, see Alan Gregg, "Mental Health," 17 Mar 1932, RFA, RG 3, S 906, B 2, F 19 For a revealing insight into how Gregg actually used the award of fellowships administratively, see A Gregg to S Cobb, 26 May 1932, RFA, RG 1 1, S 200, B 90, F 1080

[51] See *Rockefeller Foundation Annual Report for 1932*, pp 212–13, cf Penfield, *Difficult Art of Giving*, pp 232–49, 262–63

[52] For memorandum, see Gregg, "Mental Health." For the discussion with Cobb, see Gregg's office diary entry for 30 Mar 1932

[53] "Extract from agenda for Special Trustees Meeting 11 April 1933 DR 469," RFA, RG 3, S 906, B 1, F 8

[54] "The Medical and Natural Sciences Cooperative Program—Extract from Interim Report Presented to Trustees 13 December 1933 DR 475," RFA, RG 3, S 906, B 1, F 8

Louis.[55] More significantly, he was also able to obtain a commitment of $80,000 for Stanley Cobb's critically important experiment at Massachusetts General Hospital. Cobb was prepared to switch from neurology to psychiatry in order to establish a new kind of department and a new kind of hospital service in which psychiatry would be brought "into close relation with other branches of medicine."[56] Gregg privately acknowledged that this development was particularly exciting because Cobb, a widely recognized and very influential figure in American neuropsychiatry, had already established a relationship with psychiatrists in Boston which was "friendly and cooperative." "A not unimportant item of this relationship," Gregg stated, was that Cobb understood and was tolerant of those who were "emphasizing the contributions of psychoanalysis to the understanding of mental derangement."[57] Thus, by his choice of projects and people, Gregg was able to impress the trustees, satisfy President Max Mason, and, at the same time, return to the careful interdisciplinary agenda he had worked out under Pearce at the end of the twenties.

In 1934 and 1935 Gregg was able to expand his program in psychiatry significantly by shrewdly adapting to a changing political climate within the Foundation. The change in climate resulted from the fall of Max Mason and the rise of Raymond B. Fosdick within the Foundation orbit. Mason had faced difficulties with the trustees for some time, and by 1934 he had lost their confidence.[58] An interim period of great confusion followed in which Fosdick, J. D. Rockefeller, Jr.'s legal counsel, and for some time the leading Foundation trustee, acted as de facto co-president. Fosdick formally assumed the presidency on 1 July 1936. Because Gregg already enjoyed Fosdick's confidence, the program in psychiatry flourished. A special trustees' "Committee of Appraisal" chaired by Fosdick in December 1934 endorsed Gregg's leadership as "sane and balanced," acknowledged the appropriateness of his focus on psychiatry, and specifically noted that he could choose to support a wide range of practical activities in "preventive and corrective therapy . . . without prejudice to special techniques."[59] Gregg seems to have taken this last statement to mean de facto approval of his support for certain forms of psychoanalytical psychotherapy.[60] Gregg was also allowed some freedom from the strictures of overly tight linkage with the Division of Natural Sciences, and he responded with project proposals that moved clearly in the direction of teaching (as opposed to research), the direct provision of psychiatric services, and broadly conceived investigative projects that shifted the center of gravity of his division away from the biological orientation that had dominated under Mason.[61]

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[55] *Rockefeller Foundation Annual Report for 1933* (New York, 1933), pp 166–69

[56] *Ibid*, pp 170–71

[57] Office diary entry for 26 Jan 1933

[58] Personal diary entry for 16 Feb 1934, Gregg, NLM, B 5

[59] "Extract from Report of Committee of Appraisal—Presented Trustees Meeting 11 Dec 1934 DR 486," RFA, RG 3, S 906, F 8

[60] See office diary entries for 7 Dec 1934 and 9 Jan 1935

In dramatic fashion, the trustees matched their general enthusiasm with budgetary support. In 1934 they approved a total allocation for psychiatry and related fields of $497,134 (up from $361,293 in 1933).[62] In 1935 they increased their support by $1,336,484. Included in this extraordinary sum was a renewal grant to the Department of Psychiatry at Massachusetts General Hospital, where Stanley Cobb was now explicitly adding psychoanalysts to his staff with Rockefeller money and Gregg's approval.[63] Also included was a three-year award of $168,000 to the University of Chicago for a new "subdepartment" of psychiatry.[64] According to the approved plan, this division would be a multidisciplinary venture in neuropsychiatry administratively organized as a unit within the Department of Medicine but led intellectually by individuals such as former neurologist Roy R. Grinker, who had recently returned to Chicago from Vienna after undergoing analysis there with Freud (on a Rockefeller fellowship). Gregg urged Grinker to study Cobb's department as "something of a model" and also expected him to establish friendly relations with Chicago's leading psychoanalyst, the recent emigré Franz Alexander.[65] Since Gregg was at a high-point of his power and influence, he naturally thought in bold terms—of a citywide plan for psychiatry in Chicago in which Grinker and Alexander would each have important roles, Grinker's in fact being much like Cobb's in Boston. To move this scheme forward, in 1935 Gregg, simultaneously with the University of Chicago's grant, orchestrated a three-year Rockefeller grant of $100,000 to Alexander's Chicago Institute for Psychoanalysis.

Alan Gregg was now pushing his agenda boldly forward. From 1925 through 1935 he had been attempting to arrange an interdisciplinary amalgam in neuropsychiatry. This amalgam was to be dominated by the biological sciences but interspersed with psychoanalysis. To blend in psychoanalysis most effectively, analysts would need to have research interests, university connections, and linkages as close as possible to medicine's advancing scientific frontier. In Gregg's view, Alexander looked like a compatible and cooperative sort. He was a psychoanalyst respectable enough to have the confidence of his analytical colleagues, an intellectual educated broadly enough to appreciate the values of scientific investigation, and a pragmatist supple enough to fit himself into Gregg's program requirements.

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[61] On Gregg's new "relative freedom," see M Mason to Dr Strode, 9 May 1934, RFA, RG 3, S 906, F 4

[62] Budgetary data are compiled from Gregg's appendix to his 1941 memorandum, "What is Psychiatry?" RFA, RG 3, S 906, B 2, F 19

[63] Office diary entry for 3 May 1934 See also S Cobb to A. Gregg, 12 Feb and 4 Mar 1935, RFA, RG 1 1, S 200, B 90, F 1082

[64] *Rockefeller Foundation Annual Report for 1935* (New York, 1935), p 77

[65] For the expectations about Grinker's role in Chicago, see Gregg's office diary entries for 4 May 1934 and 4 Mar 1935 Cf Roy R. Grinker, *Fifty Years in Psychiatry* (Springfield, Illinois Charles C Thomas, 1979), chap 2

Gregg and Alexander had first met in Berlin in December 1929.[66] At that time Alexander was already a leading European analyst, well respected by Freud and associated with the institutionally innovative Berlin Psychoanalytic Institute.[67] He was the son of a distinguished Hungarian academic, Bernard Alexander, who was professor of philosophy at the University of Budapest and editor of Hungary's *Journal of Philosophy*. Loyal to his father's values, Franz Alexander developed a deep commitment to the open-ended pursuit of "truth" and to the life of the mind disciplined by the rigors of university scholarship. From Gregg's perspective, Alexander had several additional attractions: he had studied medicine (he received his M.D. from Budapest in 1913), worked in the laboratory of physiologist Max Verworn at Göttingen, published articles on "brain metabolism," and investigated blood chemistry while on the staff of the University of Budapest's psychiatric hospital. In 1919, at the age of twenty-eight, Alexander left Budapest for Vienna, then Berlin, and became the first student of the newly founded Berlin Psychoanalytic Institute. In 1921, after completing his training analysis with Hanns Sachs, he was awarded a prize by Freud for his essay "Castration-Complex and Character" and became an assistant at the Berlin Institute. By 1926 he was a popular training analyst at Berlin, especially appealing to visiting American psychiatrists. Alexander believed that on becoming an active member of the psychoanalytical movement he had sacrificed all hopes of an academic career. But the opportunity for an academic post soon beckoned from the United States.

Alexander first left for America in the spring of 1930. He had been invited to the International Congress on Mental Hygiene, held in Washington, D.C., in May, through the intervention of Dr. William Healy of the Judge Baker Foundation in Boston. Healy was a proponent of psychoanalysis and the leading American student of juvenile delinquency; he had visited Alexander in Berlin in the late twenties to discuss psychoanalytical approaches to criminology, the subject of Alexander's second book, *The Criminal, the Judge, and the Public: A Psychological Analysis,* which he wrote with Berlin attorney Hugo Staub.[68] Alexander's visit led to an invitation from Robert M. Hutchins, the former dean of the Yale Law School and the new president of the University of Chicago.

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[66] Office diary entry for 5 Dec 1929 Cf Franz Alexander's description of the same meeting from his own perspective in *The Western Mind in Transition* (New York: Random House, 1960), pp. 91–92

[67] The principal biographical sources for Alexander's life and career are the following Franz Alexander, *Western Mind in Transition, idem,* "A jury trial of psychoanalysis," *J Abnorm Soc. Psychol*, 1940, *35* 305–23, *idem,* "Recollections of Berggasse 19," *Psychoanal Quart*, 1940, *9* 195–204; Martin Grotjahn, "Franz Alexander A Pioneer of Psychoanalysis," in *Psychoanalysis and the Human Situation,* ed. Jessie Marmorston and Edward Stainbrook (New York: Vantage Press, 1964), pp 17–41; Pollock, "Chicago Institute of Psychoanalysis", Edna A. Lerner, "Franz Gabriel Alexander," *DAB,* suppl 7, 1961–65 (New York Scribner's, 1981), pp 5–7 Unfortunately, Alexander's personal papers—some at the Library of Congress and some at the Chicago Institute for Psychoanalysis—are not yet open for scholarly examination Except as otherwise and specifically noted, biographical details have been distilled from the sources noted above.

In Chicago, Hutchins asked Alexander to become visiting professor of psychoanalysis for a one-year period. Alexander took up this post later in 1930 but soon discovered that the social climate during the extended stay was to be shockingly different from the cordial welcome he had initially received. The medical faculty proved particularly hostile; his friendliest auditors were social scientists, a few philosophers and legal scholars, and the lay public.

When his university contract was not renewed, Alexander left for Boston, where William Healy offered him a one-year position supported by the Rosenwald Fund. His work with Healy focused on the "actual psychoanalysis of offenders" and in 1935 resulted in a book, *Roots of Crime*. In 1932 Alexander was invited back to Chicago to help create a psychoanalytical institute on the Berlin model. Not the university this time but a group of private citizens generated the local support for Alexander's renewed efforts in Chicago. On 3 October 1932 the Chicago Institute for Psychoanalysis opened its doors with Franz Alexander as director and a staff consisting of Karen Horney (brought by Alexander from the Berlin Psychoanalytic Institute as associate director), Thomas French (an American psychiatrist who had been analyzed in the twenties by Alexander in Berlin), Helen McLean (wife of Dr. Franklin McLean, director of the University of Chicago's medical clinic), and Catherine Bacon.[69]

One of Alexander's first major tasks was to mobilize support for the new institute. In this connection a meeting was arranged in Chicago in February 1933 with Alexander, Alfred Stern (head of the Chicago-based Rosenwald Fund and an enthusiastic backer of Alexander's Institute), and Alan Gregg of the Rockefeller Foundation. Gregg had been carefully following Alexander's career and subtly connecting the swings of the emigré analyst's fortunes with emerging plans for neuropsychiatry at the University of Chicago.[70] Thus, when Gregg sat down with Alexander and Stern in February 1933, he was already keenly aware of the situation in Chicago. He made this assessment of the drawbacks and opportunities. "Told A. I thought there was no likelihood of aid from the RF this year and that at any time we would prefer to see some sort of univ. connection for the Inst. . . . and I think its members have plenty of winnowing to do before they know enough to have something definitely to contribute to the study of somatic diseases. . . . I have not yet seen included what makes the simplest and to me the best case for psychoanalysis."[71] It was apparent that Alexander would have to think seriously not only about university affiliation but also about directing some of his energies toward research rigorously linking psychoanalysis with somatic pathology if he expected to interest Gregg in working to arrange Rockefeller Foundation support.

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[68] On Healy, see George E Gardner, "William Healy 1869–1963," in *Psychoanalysis, Psychotherapy, and the New England Medical Scene, 1844–1944,* ed George Gifford (New York. Science History Publications, 1978), pp 251–72 The title of Alexander and Staub's book is, of course, that of the English translation by Gregory Zilboorg (New York. Macmillan, 1931), the original German version was published in Berlin in 1929

[69] Pollock, "Chicago Institute for Psychoanalysis," pp 114–16, 125, nn. 3–5

[70] Office diary entries for 8 May 1931 and 4 Apr 1932

Alexander was already interested in this linkage to some extent. During his university and medical school days he had been fascinated by the connections between brain biology and mental expression. Later, during his association with the Berlin Psychoanalytic Institute, Alexander resorted to the classical Freudian explanation of somatic symptom formation in cases of hysteria. Conversion symptoms, Alexander argued, are the ego-exonerating "shift to the physical" of intrapsychic struggles between unconscious urges and the mechanisms of repression.[72] Moreover, the somatic symptoms of hysterical patients simultaneously symbolize the meaning of the repressed tendencies, satisfy the neurotic need for self-punishment, and achieve the secondary gains of illness.

Alexander's psychoanalytical formulations were as noteworthy in the twenties for what they did not say as for what they did say. Contemporary psychoanalytical theorists like Ernst Simmel, Felix Deutsch, Sandor Ferenczi, and George Groddeck had already gone well beyond restating Freud's original ideas to produce generalized theories of "displacement reactions," "erotization," and "symbolic meaning" extrapolated to the entire range of organic diseases.[73] Indeed, George Groddeck in a 1926 essay published in Freud's *Internationale Zeitschrift für Psychoanalyse* claimed that physical symptoms in *any* organic disorder can be understood as the result of the symbolic play of the id on the plastic body through primary process, just as the imagery of dreams can be understood as the "work" of the unconscious on day residues and other visual tracings[74] By contrast, Alexander steered clear of these extreme ideas, except to note in passing that they might one day lead to a generalized theory of both organic and psychic illness.[75] Quite possibly, Alexander's earlier commitment to experimental research kept him from offering more than faint praise for what to others in the Freudian inner circle had already become the object of sometimes extravagant enthusiasm

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[71] *Ibid*, 14 Feb 1933

[72] Franz Alexander, *The Psychoanalysis of the Total Personality,* trans Bernard Glueck and Bertram D Lewin (New York. Nervous and Mental Disease Publishing, 1930), pp 40–43 This is a translation of Alexander's *Psychoanalyse der Gesamt-personalichkeit* (Berlin, 1927), the published version of his lectures at the Berlin Institute

[73] For useful accounts of these developments, see Martin Grotjahn, "George Groddeck and his teachings about man's innate need for symbolization," *Psychoanal Rev*, 1945, *32* 9–24, Paul Federn, "Sandor Ferenczi," *Int J Psychoanal*, 1933, *14* 467–85, Robert C Powell, "Healing and Wholeness Helen Flanders Dunbar and an Extra-Medical Origin of the American Psychosomatic Movement, 1906–1936" (Ph D diss, Duke University, 1974), chaps 1 and 2, Harold I. Kaplan and Helen S Kaplan, "An historical survey of psychosomatic medicine," *J Nerv Mental Dis.,* 1956, *124*· 546–68, and Herbert Weiner, "Psychosomatic Medicine and the Mind-Body Problem in Psychiatry," in *The Handbook of the History of Psychiatry,* ed. Edwin R. Wallace and John Gach (New Haven, Connecticut. Yale University Press), forthcoming

[74] Groddeck, "Traumarbeit und Arbeit des Organischen Symptoms," *Int Zeit Psychoanal*, 1926, *12* 504–12

In the United States, Alexander became more expansive about psychosomatic theory. Medical interest in this area had recently intensified greatly, and Alexander may well have sensed an opportunity to make his mark with American physicians.[76] In his lectures at the University of Chicago he included a discussion of "psychogenic factors in organic diseases."[77] The hostile and sometimes outraged reaction of his Chicago medical auditors, especially of one intemperate experimental physiologist, suggests that he may at first have gone rather lightly over the physiological material. When he later prepared his lectures for publication during his year in Boston, he was careful to include at least plausible hypotheses about neurophysiological mechanisms.[78] In this way he hoped to link analytically probed psychodynamics with scientifically understood somatic events.

Alexander centered his first published discussion of organic psychogenesis on peptic ulcers.[79] His primary claim was that vague speculation about "psychic stimuli" or "mental conditions" in the precipitation of certain "cryptogenic" diseases needed to be replaced by more precise psychological formulations and the hunt for mechanisms specifically connecting particular psychic stimuli with related organic responses. In peptic ulcers, the central etiological problem could be reduced to discovering the cause of chronic gastritis, since chronic gastritis can in turn be related to chronic hyperacidity as well as to changes in the stomach's blood supply. Because the neurosurgeon Harvey Cushing had recently demonstrated that certain operative procedures on the brain could lead to acute perforation of the stomach (presumably by parasympathetic stimulation caused by the surgery), it was easy to assume by analogy that direct psychological stimuli in the cortical region of the brain could lead to gastric ulcer by chronic irritation of subcortical centers Alexander's hypothesis was as follows:

> Knowing from Pavlov's studies the dependence of the secretive functions of the stomach upon psychological stimuli, we understand that the permanent unconscious longing for dependence and being loved in an infantile way can exert a

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[75] Alexander, *Psychoanalysis of the Total Personality*, pp xviii, 141–42

[76] As one indication of the great upsurge of interest in psychogenic factors on the part of American physicians in the late twenties and early thirties, see the series of papers on "psychic and emotional factors in disease" published in *JAMA*, 1927, *89* 1013–25 Alexander's reference to these concerns in his widely publicized Harvey Lecture ("Psychoanalysis and Medicine"), delivered at the New York Academy of Medicine on 15 Jan 1931 and subsequently published in *Mental Hygiene*, 1932, *16* 63–84, may be read as his nod to American interests

[77] The quoted phrase is the title of chap 4 of Alexander's *Medical Value of Psychoanalysis* (Boston W W Norton, 1932)

[78] Alexander, for example, offered a biogenetic explanation for the evolution of autonomic nervous control See his *Medical Value of Psychoanalysis*, pp 178–79

[79] *Ibid*, pp 186–96 Cf his closely related lecture read to the Central Neuropsychiatric Association, Rochester, Minnesota, on 7 Oct. 1932, which was published in *JAMA*, 1933, *100* 469–73

> chronic stimulation on the stomach secretion, the result of which may be chronic hyperacidity. The stomach, exposed to permanent psychic stimulus (unconscious phantasies of being fed), behaves even when it is empty as if it were in a state of digestion and thus is exposed all the time to the influence of the gastric juice. But in this situation a chronic hyperacidity rests on a nervous basis. In this way, psychoanalysis is able to give the expression "nervous basis," a more concrete content by discovering the detailed psychic stimuli, the unconscious tendencies, which are frequently responsible for the permanent disturbance of certain organic functions.[80]

To complete the etiological sequence, chronic disturbance in function could eventually lead to local morphological change, hence peptic ulcer.

Thus, before meeting with Gregg to discuss a Rockefeller grant, Franz Alexander had already begun to amalgamate psychoanalytical and physiological approaches. He first made "psychic stimuli" specific by explicating them in terms of unconscious wishes and their repression, and he then suggested a particular neurophysiological pathway which conveyed the psychic stimulus from the cortex to the midbrain and from thence to the stomach via parasympathetic stimulation. His hypothesized mechanism was, of course, still quite vague. It also rested neither on multiple clinical instances nor on controlled experiments. Furthermore, at this point Alexander's thinking was still conceptually unformed, because in one passage he simply lumped "organ neurosis" with classic hysteria and treated both with the physiological vagueness common in contemporary psychoanalytical literature.[81] In another passage he momentarily flirted with Walter B. Cannon's refined experimental physiology of emotional expression, but he then relegated most of his consideration of Cannon's work to one, largely negative, footnote.[82] Alexander's attempt to cultivate Alan Gregg in order to secure Rockefeller Foundation funding for the Chicago Institute would very soon, however, significantly influence his approach to psychosomatic problems.

Alexander's not always subtle reorientation during his courtship of Gregg can be demonstrated concretely in the correspondence between the two in 1933 and 1934. Seven months after his meeting with Gregg in February 1933, Alexander wrote to inform him: "We are going to concentrate the work of the Institute so far as possible this coming year in studying in an organized fashion the relationship between the somatic and psychogenic factors in somatic disturbances."[83] In January 1934, Alexander acknowledged receipt of a "very interesting and stimulating letter" from Gregg and then commented further:

> Regarding your admonition that I have not considered the well known effect of fear upon the control of the sphincters and on the peristalsis, I wish to tell you

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[80] Alexander, *Medical Value of Psychoanalysis,* pp 193–94
[81] *Ibid.,* pp 184–85
[82] *Ibid.,* pp 185, 202–3
[83] F Alexander to A. Gregg, 19 Sept. 1933, RFA, RG 1 1, S 216, B 3, F 26

> that . . . I did not deal here with the reflex reactions to fear of the circulatory and alimentary system because their psychological analysis is extremely difficult. In general one can say that fear mobilizes all kinds of active aggressive mechanisms of the individual as Cannon has shown it, and I consider the eliminating functions belong psychogenetically into the category of active aggressive mechanisms serving for defense against external attacks. . . . Your constructive criticism has now given me the stimulus to include into my article a paragraph covering the same material as this letter.[84]

Alexander was clearly being prodded by Gregg to pay increased attention to the physiology of the emotions, and as a result he was beginning to think seriously about how best to incorporate Cannon's perspective into his psychosomatic investigations.

By June 1934, Alexander had actively redirected the Chicago Institute for Psychoanalysis. As announced in early public statements, the Institute's original priorities were to "extend itself to the application of analysis to general medical, educational, and sociological problems, including the necessary research."[85] These rather general goals were reflected in an early lecture series in the spring of 1933, which included the following topics. "Psychic Factors in Crime" (Alexander); "The Mother's Conflicts as Expressed Toward the Child" (Horney); "Psychological Aspects of the Economic Depression" (Blitzsten).[86] By summer 1934, the Institute had begun to function rather differently. Dr. Horney had left for New York after a stormy year in Chicago (possibly due in part to subtle pressure from the Rockefeller Foundation), and Drs. Karl Menninger, Leon Saul, and George Wilson had joined as "clinical associates."[87] With the advice of a board of medical consultants, detailed plans had been drawn up for systematic research into gastrointestinal disorders as well as for future research on "psychological factors involved in ailments of the respiratory tract, hypertension, endocrine gland disturbances, and skin disorders."[88] A start had been made at presenting case records in a systematic fashion, and the growing and shifting Chicago Institute staff was generally reorganizing as a research team focused to a large extent on psychosomatic problems.[89]

Alexander arranged an exhibition of the Institute's new research orientation in a long series of articles on "gastrointestinal disturbances" published in the October 1934 issue of the *Psychoanalytic Quarterly*.[90] When this new journal first began publication in 1932 as a deliberately orthodox

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[84] F Alexander to A. Gregg, 22 Jan 1934, RFA, RG 1 1, S 216, B 3, F 27

[85] RFA, RG 1 1, S 216, B 3, F 25

[86] RFA, RG 1 1, S 216, B 3, F 26

[87] For the staff of the Institute in 1934, see p 4 of the memorandum written by Alan Gregg and dated 1 May 1935 (RFA, RG 1 1, S 216, B 3, F 28) As for possible though subtle Rockefeller Foundation pressure for the personnel shifts taking place at the Chicago Institute, see A. Stern's letter to Gregg, 26 June 1934, RFA, RG 1 1, S 216, B 3, F 27

[88] Stern to Gregg, 26 June 1934, RFA, RG 1 1, S 216, B 3, F 27

[89] "Institute for Psychoanalysis Summary for 1934–1935," dated 23 Oct. 1935, RFA, RG 1 1, S 216, B 4, F 4

[90] *Psychoanal. Quart*, 1934, *3*· 501–88

American organ of the international psychoanalytical community, it was careful to include an abstract of Ernst Simmel's recent case report "The Psychogenesis of Organic Disturbances and Their Psychoanalytic Treatment."[91] In 1933 it printed an English version of Felix Deutsch's synthetic "Studies in Pathogenesis: Biological and Psychological Aspects."[92] Both Simmel's and Deutsch's contributions were conceived very much in the spirit of George Groddeck. Alexander and his Chicago colleagues now took to the pages of the new journal to advance their own program and simultaneously to distinguish themselves from those analysts who still clearly identified with the extreme psychogenic position.

In his lead article Alexander first criticized the inadequacies of earlier psychoanalytical approaches, claiming that they were often internally inconsistent from a logical point of view and simply implausible in the assertions they made about the direct somatic influence of "fantasies or wishes."[93] It was important to recognize the crucial *differences* between classic hysteria and those disturbances of organic function controlled by the "vegetative" nervous systems. Thus,

> whereas in conversion hysteria the unconscious tendency finds a direct expression in the physical disturbances, in organic processes controlled by the vegetative nervous system often a longer chain of intermediary physiological processes is interpolated between psychological stimulus and organic end-result It is a methodological error to attempt to interpret psychologically an organic symptom which is the end-result of an intermediary chain of organic processes[94]

Having made this essential distinction between conversion phenomena and "vegetative neuroses," Alexander developed his own alternative psychogenic hypotheses. In cases of gastrointestinal disturbance, each basic "type"—"gastric," "colitis," or "constipation"—is associated with a specific unconscious psychological conflict that as stimulus is mediated through the autonomic nervous system before evoking its specific organic response. In the gastric type, repressed oral-receptive tendencies (the unconscious wish to receive or be fed) serve as "chronic psychic stimuli of the stomach independent of the process of nutrition."[95] In the colitis type, the repressed desire to be active and aggressive in an ejective way "serves as permanent irritation of the peristalsis independent of digestive functions."[96] And in the constipation type, the unconscious rejection of the obligation to give provides the "chronic inhibitory stimuli leading to constipation."[97] In all three types, an "elementary tendency" of intaking, retaining, or eliminating is inhibited and "expressed through the vegetative system instead of the voluntary system," and that vegetative "expression" triggers the particular gastrointestinal response.[98]

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[91] The abstract of Simmel's paper, prepared by Bettina Warburg, was published in *Psychoanal Quart*, 1932, *1* 166–70

[92] Deutsch's paper appeared in *Psychoanal Quart*, 1933, *2* 225–43

[93] Alexander, "General principles, objectives, and preliminary results," *Psychoanal Quart*, 1934, *3* 501–39.

[94] *Ibid*, p 505

[95] *Ibid*, p 537

[96] *Ibid*

[97] *Ibid*

By referring thus to instinctual drives and their unconscious repression, Alexander clearly demonstrated his continuing loyalty and commitment to the psychoanalytical tradition Yet, at the same time, by sharply distinguishing between those functions under voluntary and those under autonomic nervous control, he deftly separated from Simmel and Deutsch and placed himself on the side of up-to-date neurobiological science. His new model —a generalization and extension of his earlier ideas about peptic ulcers— rested on the fundamental assumption of a necessary "chain of intermediary physiological processes ... interpolated between psychological stimulus and organic end-results."[99] Stepwise physiological mechanisms, therefore, had to be factored into all cases, and in some they provided useful alternative formulations.

By March 1935, Gregg was prepared formally to propose a three-year grant of $100,000 for the Chicago Institute to the Rockefeller board. As noted above, at this time he was at a high point of his power and influence within the Foundation and had gained the authority to begin implementing his broadly conceived interdisciplinary plan for psychiatry. The Rockefeller Foundation had already begun to fund Stanley Cobb's neuropsychiatry service at Massachusetts General Hospital and was preparing to provide a grant of $168,000 to the University of Chicago for a new "subdepartment" of psychiatry in which former neurologist Roy Grinker was expected to play a major role. Gregg anticipated that Grinker would be on good terms not only with his colleagues in the university but also with Franz Alexander at the Chicago Institute for Psychoanalysis. Since, according to Gregg, Alexander was now willing to place special emphasis on "the correlation of medical and physiological problems with the findings of psychoanalysis," a clever, citywide plan for Chicago seemed in the offing, even though Alexander had not yet obtained formal university affiliation for his Institute.[100] In any event, Gregg was now ready to proceed with some optimism after what had been many months of studied delay.

Gregg's argument in his draft resolution to the trustees merits consideration. First he addressed the question of the place of psychoanalysis in psychiatry. He claimed that "psychoanalysis occupie[d] an important part of the training of psychiatrists" but that under present conditions, training analyses were costly and unsatisfactory "due to the fact that ... [they were] still on a non-institutional basis."[101] The Chicago Institute was the country's institute most promising as a major locus for training because of the quality of its leadership and because of its growing integration with the universities and other professional organizations in the city. Support for Alexander and his staff would keep from drifting into income-generating private practice "a valuable group of teachers and investigators [who would be] held at teaching which is much in demand and at research which is critically important for the validation of a significant school of psychoanalytic activity."[102] Scientific study would aim at "systematic comparative research on a series of similar cases" and would focus in large part "along psychophysiological lines." In short, "few procedures could more promisingly help to sift out the valid portions of psychoanalytic theories than one which enabled them to be tested in an institution instead of in private consultation rooms. This would favor comparison, exchange of opinion, verification and a correlation with the findings of internal medicine, surgery and the more orthodox views of psychiatry."[103] Persuaded by Gregg's powerful and compelling argument, the Rockefeller board approved the grant on 10 April 1935.

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[98] *Ibid.*, p 535

[99] *Ibid.*, p 505

[100] The quoted phrase is from p 4 of Gregg's draft authorizing resolution, 4 Mar 1935, RFA, RG 1 1, S 216, B 3, F 28

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During the first year and a half of the grant, Alexander made every effort to please Gregg and the Rockefeller Foundation. Lectures, seminars, case conferences, and training analyses ran smoothly, while the Foundation and the Institute cooperated good-naturedly in the review and oversight of fellowship awards. Grinker, in fact, was directly involved in the review of candidates for training analyses.[104] The Institute also reached out successfully to the Chicago medical community, and in late 1935 arranged an impressive lecture series that included presentations by the University of Chicago's Walter L. Palmer on the physiology and clinical pathology of peptic ulcers, ulcerative colitis, and functional bowel disturbance; R. W. Gerard on recent progress in the physiology of the nervous system; and Roy Grinker on the clinical pathology and theory of epilepsies.[105] Most important from Gregg's point of view, Alexander kept the Institute staff focused on its own psychosomatic research program. Alexander himself pushed away even more decisively from the symbolistic psychoanalytical alternative to physiologically mediated psychogenesis, and in 1936 he succeeded in recruiting the endocrinologically experienced analyst Therese Benedek.[106] He also led efforts to sharpen case records, data collection, and experimental design.[107] Finally, with Gregg's active cooperation and approval, he began conducting clinical studies on asthma and encouraged staff member Leon J. Saul to initiate research with advanced electrophysiological research techniques.[108]

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[101] *Ibid*, p 1

[102] *Ibid*, p 5

[103] *Ibid*, pp 2–3

[104] See letter from R.A. Lambert to A. Stern, 26 Nov 1935, RFA, RG 1.1, S 216, B. 3, F 29.

[105] From an announcement labelled "A Special Course for Physicians Engaged in Research Work on Psychogenic Organic Disturbances to be given at the Chicago Institute for Psychoanalysis," RFA, RG 1 1, S 216, B 3, F. 29

[106] Alexander's decisive distancing from the symbolistic approach was clearly expressed in the second (1936) edition of *Medical Value of Psychoanalysis*, a portion of which was also published in *Psychoanal Quart.*, 1936, *5* 548–49 With regard to Benedek, see Gregg's office diary entries for 13 Jan. and 16 Apr 1936

But as time passed, tensions developed between Gregg and Alexander. Affiliation with the University of Chicago failed to materialize. Moreover, with Rockefeller money now secured and the program for the Institute set, Alexander apparently felt free to head off in what Gregg regarded as a centrifugal direction—back toward political and "sociological" concerns such as he had pursued in Berlin and in his early days in the United States. He seems to have been motivated in part by the shape of world affairs, in part by the warmth of his reception among University of Chicago social scientists, and in part by the urgency he and his staff felt to achieve greater public recognition (and income) in Chicago[109] Gregg thought it important to exercise centripetal restraint by calling Alexander's attention back to science, training, and research. At first he operated subtly behind the scenes, as when a proposed lecture series on political and social topics—originally scheduled for the fall of 1936—was indefinitely postponed, to Gregg's obvious relief.[110] But as Alexander continued to pull away, Gregg first withheld personal support and then started to write letters in which his skeptical and unenthusiastic attitude clearly came through.[111] With some variations in intensity, the tension between Gregg and Alexander continued throughout the next year and a half. When, in November 1938, Alfred Stern sent Gregg a copy of his letter sharply rebuking Alexander and the Chicago Institute staff for sliding again into political concerns, Stern knew that he would find a receptive audience at Rockefeller Foundation headquarters.[112]

Gregg's growing discomfort with Alexander's recurrent "sociological" deviations coincided chronologically with his increasing skepticism about the scientific rigor and even the logical validity of the Chicago Institute's approach to psychosomatic research. Thus it seems no accident that on 27 November 1936 Gregg recorded in his office diary Alexander's resurfaced inclination to apply psychoanalytical thought to social and political conditions and at the same time noted with at least a touch of disbelief that the Institute's new research on asthma was based on the "characteristic assumption" of an "early instinct or desire to be protected." Gregg added with uncommon irony:

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[107] For Alexander's efforts at improving case records and data collection, see his memorandum dated 23 Oct. 1935 labelled "Institute for Psychoanalysis Summary for 1934–1935 Board of Trustees Meeting," RFA, RG 1 1, S 216, B 4, F 4 An example of his attempt to improve experimental design is his paper with George Wilson, "Quantitative dream studies a methodological attempt at a quantitative evaluation of psychoanalytic material," *Psychoanal Quart*, 1935, *4* 371–407

[108] Office diary entries for 30 Apr 1935 and 27 Nov 1936

[109] For Alexander's felt need for greater public recognition (and income), see the letter from A. Stern to T Blake, 9 July 1936, RFA, RG 1 1, S 216, B 3, F 31

[110] See note from A. Stern to A. Gregg, 15 Aug 1936, RFA, RG 1 1, S 216, B 3, F 31

[111] On Gregg's withholding as perceived by Alexander, see F Alexander to A. Gregg, 9 Feb. 1937, RFA, RG 1 1, S 216, B 3, F 32 As an example of Gregg's skepticism, see A. Gregg to F Alexander, 24 Mar 1937, RFA, RG 1 1, S 216, B 3, F 32

[112] Stern's letter to the Institute is dated 7 Nov 1938 (RFA, RG 1 1, S 216, B 3, F. 35) In an unusually quick response, Gregg wrote to Stern on 9 Nov 1938 as follows "I have as you probably know indicated to Doctor Alexander that emphasis upon the political and social inferences which can be made from certain psychoanalytical interpretations seem to me to be in the more dubious and less reliable group of services which any research institute in the psychological field can offer" (RFA, RG 1 1, S 216, B 3, F 35)

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> On request told Alexander that it seemed characteristic of Freudian explanations as well as some other to endeavor to ascribe things to causes which operate so early in life that no one can deny that they may have been operative–e.g you stutter because you were frightened as a child—no not as a child, as an infant, and then some one comes along and says that the real difficulty occurred before birth, leaving the heavy burden of proof (other than presumptive) to those who said it did not.[113]

Alexander's response to Gregg's more critical stance was to revive his earlier attempts at ingratiation. But now his efforts took on a laboratory focus. In March 1937, Alexander transmitted a proposal to Gregg calling for the establishment of a laboratory within the Institute.[114] The laboratory would be especially equipped for endocrinological, biochemical, and electroencephalographic investigations. Alexander himself would provide the required expertise in biochemistry, Saul in electrophysiology, and Dr. Hugo Rony, chief of the Endocrine Clinic at Northwestern, in endocrinology. All that would be needed, in addition to laboratory space and equipment, was "one young physiologist for part time and a laboratory technician." The total project would cost about $9,000 during the start-up year, roughly $8,500 of which could be provided out of the Institute's current fiscal resources. In April, Saul and Alexander presented their findings on "The Human Spirogram" to the annual meeting of the American Physiological Society.[115] They reported that the careful respiratory tracing of 265 individuals revealed characteristic features of each individual, his or her "spirogram." In June, Saul presented results of his collaboration with Hallowell Davis of the Harvard Physiology Department to the Annual Meeting of the American Neurological Association.[116] They reported a clear correlation between electroencephalographically measured alpha indices and psychoanalytically investigated personality traits. Finally, in late 1937 or early 1938, Alexander moved the laboratory project still further along by encouraging collaboration between recently appointed Therese Benedek of the Institute and Dr. Boris Rubenstein, a Rockefeller Foundation fellow in human biology at Western Reserve University in Cleveland. Benedek and Rubenstein began work on a study of "the influence of emotional factors upon the menstrual cycle and that of the menstrual cycle on emotional states."[117] Gregg remained unimpressed by all this new activity. On 2 November 1937 he remarked in his office diary: "My size-up of the situation in this Institute is that . . . the physiological correlations they are attempting are not in competent physiological hands and change is needed in this matter."[118]

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[113] Office diary entry for 27 Nov 1936

[114] See memorandum dated 18 Mar. 1937, accompanying letter of same date from Alexander to Gregg, RGA, RG 1 1, S 216, B 3, F. 32.

[115] Brief account published in *Amer J Physiol*, 1937, *119* 396–97

[116] Brief account published in *Trans Amer Neur Assoc*, 1937, pp 167–69

The upshot of this growing tension and skepticism was that while the Institute's grant was extended in 1938, the Rockefeller Foundation voted extension with some reservation and reluctance. In fact, Gregg clearly stipulated that this was the last time he would recommend funding for Alexander's operations.[119] The major problems Gregg identified were Alexander's persistent failure, despite earlier hopes, to achieve a binding affiliation with any important Chicago medical institution, the poor quality of the physiological correlations undertaken by Institute staff, and Alexander's tendency to convert the Institute and all its programs into a "one man show."[120] His excessive salary—$21,600 per annum in 1935—was also an issue.[121] Finally, Gregg and his staff began to pay increasingly detailed attention to the fine points of fellowship selection and management, thus making it clear that the Rockefeller Foundation believed Alexander's direction of the affairs of the Institute needed close surveillance.[122]

A rift had developed between Gregg and Alexander and it grew rapidly, as evidenced in an exchange of internal memoranda between Gregg's two junior associates in August 1939:

> *Dr Lambert.* . . . I still don't think much of the Chicago Institute crowd. Maybe Alexander has contributed a little something towards making psychoanalysis respectable, but he certainly has not brought it into the scientific fold I shall feel relief when the R[ockefeller]. F[oundation] grant terminates—and is not renewed.
>
> *Dr. O'Brien.* I have the same general hesitation as you about Alexander and some of the other people in Chicago. . . . Frankly, I would like to see the directorship of any institute of psychoanalysis turned over to, say, a sound physiologist or a good internist in medicine [123]

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[117] Exactly when Benedek and Rubenstein began their collaboration is a bit difficult to determine In a letter to Gregg dated 9 Mar 1938 (RFA, RG 1 1, S 216, B 3, F 34), Alexander calls attention to "*recent* developments in our work on the relation of the ovulation cycle to psychological factors" (my italics)

[118] Office diary entry for 2 Nov 1937

[119] In his office diary, Gregg recorded the following on 2 Nov 1937 "I should hardly see the necessity of RF aid beyond a three to five-year period in addition to the present grant, and that the next action RF takes should be based on a clear understanding of termination at the end of the period." The trustees' resolution officially approving the grant, dated 18 Mar 1938 (RFA, RG 1 1, S 216, B 3, F 25), clearly indicates that the $120,000 was for "the five-year period 1 Oct 1938–30 Sept. 1943."

[120] On these issues, see office diary entries for 2 Nov 1937 and 18 Sept 1939 See also A. Gregg to F Alexander, 21 Jan 1938, RFA, RG 1 1, S 216, B 3, F 34

[121] See office diary entries for 6 Dec. 1938 and 31 Oct. 1941

[122] Dr Robert A. Lambert was Gregg's assistant who dealt directly with Alexander on these issues For an indication of his growing concern, consider his letter to Alexander dated 12 Jan 1939 (RFA, RG 1 1, S 216, B 3, F 36)

[123] R A. Lambert to D P. O'Brien, 22 Aug 1939, and D P O'Brien to R A. Lambert, 30 Aug 1939, both in RFA, RG 1 1, S 216, B 3, F 37

The rift clearly underlined in these memoranda was doubtlessly reinforced by the rigorous and fruitful recent experimental work on psychosomatic interactions not only of psychoanalytically trained Jacob Finesinger in Stanley Cobb's department at Massachusetts General Hospital but also of Harold Wolff at Cornell Medical Center.[124] Moreover, in 1938 Gregg had launched an ambitious new project at Washington University in St. Louis, where he once again tried to bring together the neurological, psychiatric, and psychoanalytical elements for a new "neuropsychiatric" synthesis dominated by biology but allowing a subsidiary place for psychoanalysis.[125] These developments—both investigative and institutional—were serious, and to a large extent successful, efforts to amalgamate psychoanalysis with refined neurobiological science in ways that went well beyond what Alexander and his staff had achieved or could accomplish in Chicago. Moreover, psychoanalytical training was slowly but clearly becoming available under the auspices of university psychiatry departments.[126] If anything, by the late thirties Gregg may well have begun to feel that the balance in psychiatry had perhaps tipped too far in the direction of psychoanalysis and that a steadying influence now was needed. And then World War II descended heavily upon American academic medicine and philanthropy. For this complex of reasons, by the early forties Gregg's alliance with Alexander, for all practical purposes, was no longer operative.

In the long view, then, we can say that Franz Alexander and Alan Gregg had made and subsequently broken an alliance of mutual opportunism. They had tried to use each other, each to achieve his own purposes. In one sense both failed, in that Gregg did not quite get Alexander to do the research amalgamating psychoanalysis with neurophysiology and related disciplines which he wanted, and Alexander did not get the continuing support he desired for the Chicago Institute In another sense both succeeded, in that Alexander obtained financial support at a critical time, support that in fact clearly helped consolidate his role and that of his Institute in American psychoanalysis, while Gregg urged psychoanalysis more effectively into linkage with American psychiatry and even with American neurophysiology, although the most fruitful work of this kind (as judged by later achievements) was done elsewhere and perhaps even in contradistinction to Alexander.

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[124] Finesinger's psychosomatic work on Cobb's service is described in general terms by Benjamin V White, *Stanley Cobb A Builder of the Modern Neurosciences* (Boston: Francis A. Countway Library of Medicine, 1984), pp 212, 245–46 Some of his research findings of particular note, "Influence of pleasant and unpleasant mental content upon total ventilation, oxygen consumption, and respiratory tracings in psychoneurotic patients," were reported in *Trans Amer Neur Assoc.*, 1937, pp 143–45 Wolff and psychoanalyst Bela Mittleman published their joint study, "Experimental observations on changes in skin temperature associated with induced emotional states," in the same volume of *Transactions*, pp 136–40.

[125] The key faculty in the new department—funded for three years at $150,000—were neuroanatomist David Rioch, clinical psychiatrist John Whitehorn, and psychoanalyst Felix Deutsch All three had recently been at Harvard, Whitehorn and Deutsch specifically on Cobb's service Rioch was chair of the new department. Gregg's entry in his office diary for 14 Oct. 1940 is particularly revealing "Rioch is clearheaded about the position he is willing to have psychoanalysis assume in the department. He wants it neither omitted nor omnipresent."

[126] White, *Stanley Cobb*, p 213, claims that Helene Deutsch, after her arrival on Cobb's service in October 1935, "soon took on a number of staff members for analysis" Possibly more important, however, were the actions of a group of New York City psychoanalysts which led to the creation of a psychoanalytic institute under the aegis of the Department of Psychiatry of Columbia University's College of Physicians and Surgeons in 1944. For aspects of this story, see John A. P Millet, "Psychoanalysis in the United States," in *Psychoanalytic Pioneers*, ed. Franz Alexander, Samuel Eisenstein and Martin Grotjahn (New York Basic Books, 1966), p 559, and George E. Daniels and Lawrence C. Kolb, "The Columbia University psychoanalytic clinic: an experiment in university teaching in psychoanalysis," *J. Med. Educ.*, 1960, *35*: 164–71 The new Columbia institute was the first psychoanalytical training institute affiliated with a medical school and was also sanctioned by the American Psychoanalytic Association, although it was antedated by a training institute at Flower-Fifth Avenue Hospital which had been created two years earlier without those involved seeking approval by the Association

Another broad way of looking at the Alexander-Gregg interaction is to focus on Alexander's discovery or at least articulation of his "specificity" version of psychosomatic medicine in 1933 and 1934, with which, in some sense, he may then have been burdened for the next two decades.[127] Likewise, one might highlight Alexander's efforts to become increasingly "physiological" in 1936 and 1937. As suggested above, both of these intellectual developments were conditioned to a considerable extent by Alexander's attempt to accommodate himself to Alan Gregg at the Rockefeller Foundation. Gregg used a carrot and not a stick, yet he succeeded in eliciting these responses. Thus, American philanthropy, strictly speaking, was influencing the shape of ideas in an important corner of American medicine

Was the Rockefeller Foundation in Gregg's efforts doing anything fundamentally different from what it had done earlier in the century, for example, in its support of public health programs or medical education during the Progressive era? Although many specific elements had changed, on the most general level the Foundation continued to operate as it always had, by a process that can be described in the evolutionary sense as "artificial selection." Foundation officers handpicked specific individuals already present in a competitive population, shaped and nurtured those few who seemed to conform best to the Foundation's criteria, selectively bred the prized stock under carefully controlled conditions, and then eliminated, when necessary, those parents or offspring who, for whatever reason, failed to meet expected standards. They used their grants, in other words, to choose, mold, and reproduce the individuals and institutions the Foundation had selected for leadership.

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[127] Alexander may have been "burdened" in the sense that whatever other interests he developed, he seems to have felt that recurrent need to extend, perfect, and defend his earlier psychosomatic studies and the specificity theory on which they were based See, for example, Franz Alexander and Thomas French, *Studies in Psychosomatic Medicine* (New York Ronald Press, 1948), Franz Alexander, *Psychosomatic Medicine* (New York W W Norton, 1950), Franz Alexander, Thomas French, and George H Pollock, *Psychosomatic Specificity* (Chicago University of Chicago Press, 1968) For useful reviews of the history of American psychosomatic medicine which discuss the role of Alexander, see the references cited in n 73 above and Herbert Weiner, introduction to *Psychobiology and Human Disease* (New York Elsevier, 1977), Chase Kimball, "Conceptual developments in psychosomatic medicine, 1939–1969," *Ann Int Med*, 1970, *73* 307–16, Z J Lipowski, "Psychosomatic medicine in the seventies an overview," *J Psychiat*, 1977, *134* 233–44, Morton F Reiser, "Changing Theoretical Concepts in Psychosomatic Medicine," in *American Handbook of Psychiatry*, ed Silvano Arieti, 7 vols (New York Basic Books, 1974–81), 4 477–500, Louis A Gottschalk, "Psychosomatic medicine past, present, and future," *Psychiatry*, 1975, *38*· 334–45

Even with this continuity in Rockefeller Foundation operating style, it is probably safe to say that Alan Gregg was at no time consciously involved in the deliberate construction of a hegemonic paradigm for American medicine. Rather, Gregg's choices—like earlier choices made by other Rockefeller Foundation officers—were made with the illusion of objective neutrality and were intended self-consciously to achieve results genuinely believed to be important for science and of benefit to mankind. It is true that he used Rockefeller resources to help impose his primary disposition to interdisciplinary amalgams dominated by the experimentally grounded biological sciences, and this left little room in the fields of psychoanalysis and psychosomatics for the elaboration of analogical relationships, symbolic processes, or sociological perspectives. Yet Gregg understood his approach as, simply, the most valid and promising one possible for the medical sciences of his day. In his mind there was no other reasonable way to proceed. But to say that Gregg was, at most, minimally tolerant of alternative approaches and, at the same time, self-consciously idealistic in his goals is not the same thing as saying that his choices were uncontaminated by ideology or lacking in implications for the future. In the emerging field of psychosomatic medicine, Alan Gregg and the Rockefeller Foundation indeed made their influence felt, and in later decades—at least according to certain critics—American psychosomatics had to pay a real price for what had earlier seemed merely generous and benignly intended support.[128]

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[128] For a critic commenting perceptively on this "price," see Eric D Wittkower, "Twenty years of North American psychosomatic medicine," *Psychosom Med*, 1960, *22* 308–16

MEDICAL SCIENCE IN THIRTEENTH-CENTURY CASTILE: PROBLEMS AND PROSPECTS*

Luis García-Ballester

In his classic *Introduction to the History of Science,* George Sarton stated: "The importance of Alfonso X el Sabio's activity as a promoter of translation and scientific research can hardly be exaggerated. He might be called the founder of Spanish science; he was one of the greatest intermediaries between Arabic and European knowledge."[1] This sequence of assertions was based on research into the astronomical work of the scientific circle around the king[2] and on the fact that Alfonso was "the founder of the Spanish scientific language and literature,"[3] but it disregarded gaps in our knowledge of the nature of science in the "Wise King's" reign, gaps that still have not been closed. For example, our knowledge of the medical science available in Castile at the time is incomplete. This society, which had a fascinating ruler and existed during a decisive century, has nevertheless failed to attract the attention of historians of medicine, in Spain and abroad. No work has been done on the development of medical science in Alfonso's reign (1252–84), or indeed on any aspect of medicine in thirteenth-century Castile.[4] I am not going to be so bold as to try to fill, in only a few pages, this historiographical void, I shall simply raise a number of questions and provide foundations for their answers.

The French historian of science and Hispanicist Guy Beaujouan has

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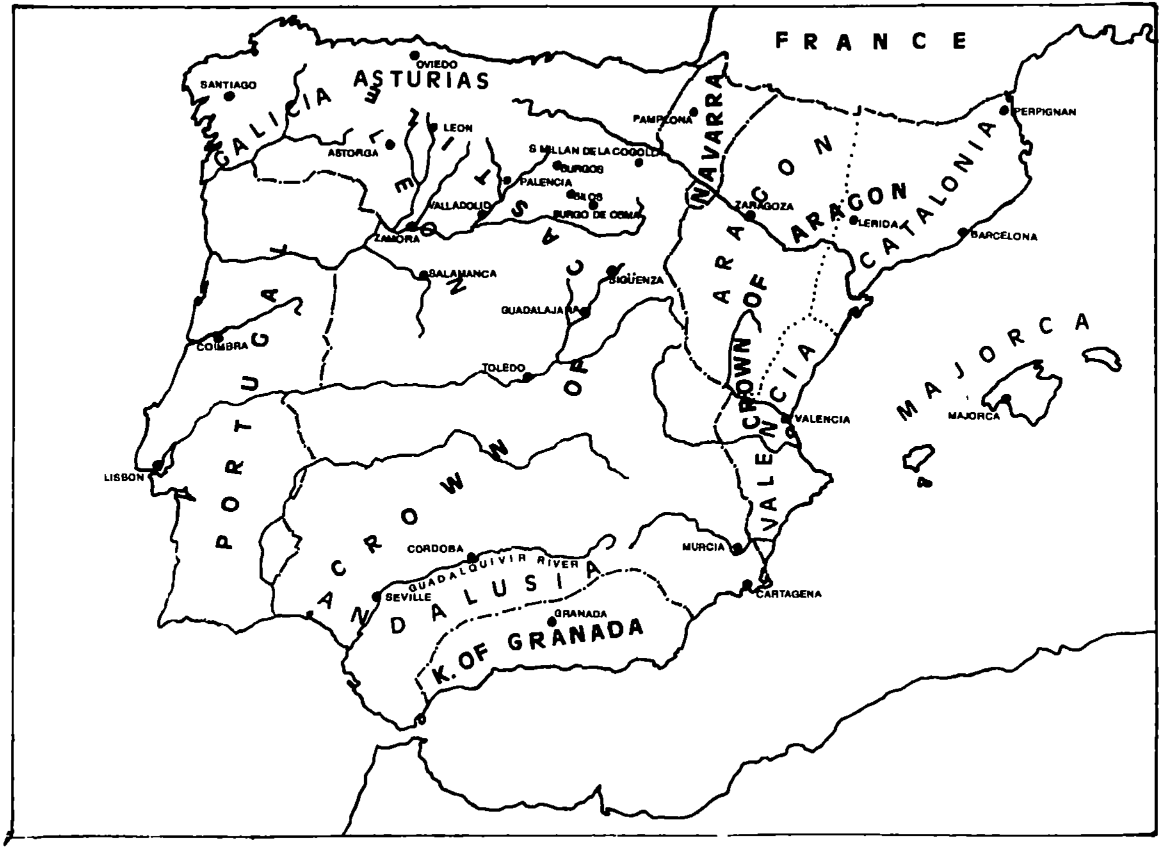
[1] George Sarton, *Introduction to the History of Science,* 3 vols. in 5 (1931; reprint, New York: Krieger, 1975), 2(2): 835.

[2] Manuel Rico y Sinobas, ed., *Libros del saber de astronomía del rey d. Alfonso X de Castilla,* 5 vols. (Madrid: Tip. de Don E. Aguado, 1863–67). A new microfiche version edited by Lloyd A. Kasten and John Nitti is *Concordances and Texts of the Royal Scriptorium Manuscripts of Alfonso X el Sabio* (Madison, Wisconsin: Hispanic Seminary of Medieval Studies, 1978). A group at Barcelona University led by professors Juan Vernet and Julio Samsó is currently studying the whole of Alfonso X's astronomical works; see Juan Vernet, ed., *Nuevos estudios sobre astronomía española en el siglo de Alfonso X* (Barcelona: Consejo Superior de Investigaciones Científicas [hereafter C.S.I.C.], 1983).

[3] "The translation ordered by him required the building up of a new vocabulary," Sarton, *Introduction,* 2(2): 839.

[4] We have only found Luis Barcia-Merayo's brief paper, "La medicina en el *Lapidario* de Alfonso el Sabio," *Medicamenta,* 1969, *52*: 221–22, which is of little value. The recent monograph by Luis Sánchez Granjel, *La medicina española antigua y medieval* (Salamanca: Universidad de Salamanca, 1981), adds nothing to the little that was already known about medicine in Castile in the thirteenth century.

Bull. Hist. Med. 1987, 61: 183-202



The Iberian peninsula in the thirteenth century. The kingdoms that occupy the territory of present-day Spain are those of Aragon, Castile, Navarre and Granada pointed to four factors that conditioned the formation of science in Spain in the late Middle Ages: the Muslim presence, the outstanding role played by the Jews, the precocious maturity of the Peninsular languages, and the weakness of Spanish universities.[5] These four factors were also operative in the field of medical science, in what we might term the dynamics of the assimilation, elaboration, and transmission of medical ideas. Specifically, they acted upon the book, the manuscript: this was the vehicle for science, which in the medical field extended from what we today call medical sciences (anatomy and physiology together in the widest sense of the terms) to theoretical and general therapeutics, passing through the complete range of pathology.

The complexities of medical practice, though intricately interwoven with the world of medical science, present another order of problems. These complexities included the practical dimensions in which the medical act was performed; the doctor's training and his performance as a professional; and the description of specific healing techniques. Furthermore, we must never lose sight of the fact that all this was carried out in a complex web involving the obvious interplay of scientific, economic, political, philosophical, religious, and other elements. An important part of this web, again, was composed of the four factors indicated by Beaujouan. Neither the level of medical ideas nor the different aspects of medical practice in thirteenth-century Castile has been studied.[6]

We must acknowledge one major obstacle to such a study, especially to the clarification of medical practice in those territories covered by the Crown of Castile in Spain in the late Middle Ages (and particularly the thirteenth century): our lack of documentary evidence. This deficiency provides perhaps the greatest contrast between Castile and the other part of Spain during this period, the Crown of Aragon (Catalonia, Aragon, Valencia, Majorca). One small detail will suffice to illustrate this point: the magnificent archives in Barcelona and Valencia enable us, for the years of the fourteenth century, to know the names of perhaps two thousand professional healers (Christians, Jews, Moors) who were practicing in a population of some eight hundred thousand inhabitants;[7] in contrast, we know the names of scarcely more than two dozen healers for the same period for the whole of the Crown of Castile (from Galicia, Asturias, Leon, to Seville and Murcia), which had a population of approximately four million. We cannot be sure how far this lack of evidence for Castile distorts our conclusions, but if we were to take the ratio between healing or medical attention and population size as an indicator, we might well ask ourselves if there was any medicine at all in Castile. Did scientific research, one of the things that has led to King Alfonso X being called "The Wise," not affect medicine?

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[5] Guy Beaujouan, *La science en Espagne aux XIVe et XVe siècles* (Paris Université de Paris, Palais de la Découverte, 1967), p 7

[6] I have elsewhere considered the role played by the Muslim minority in Christian medicine in Spain in the late Middle Ages See Luis García-Ballester, *Historia social de la medicina en la España de los siglos XIII al XVI,* vol. 1, *La minoría musulmana y morisca* (Madrid Akal, 1976-)

[7] Michael R. McVaugh and I are currently working on medicine as a medical system or organization in the Crown of Aragon during the fourteenth century

In the second half of the twelfth century, Toledo and some other parts of Castile (to which the extensive Guadalquivir valley and Murcia had not yet been added) witnessed a decisive chapter in the history of European medicine, the production of numerous translations into Latin from Arabic of the most important works in Greek and Arabic biology and scientific methodology as well as medicine. Why then is it that, barely one hundred years later, we can find no trace in Castile of the rich medical corpus translated in Toledo?[8] Here the scarcity of documents is insufficient to explain the lack of a satisfactory response.

In my current investigations into the medicine of thirteenth-century Castile, I have noticed a number of circumstances and facts that seem to me to begin to answer these questions. Let me offer some of the conclusions that I have reached.

First, the Castilian universities show no signs of scholastic medicine, while the other medieval institutions that had earlier transmitted scientific and medical knowledge (the monasteries and cathedral schools) now display interests that are old fashioned and out of date.[9] Second, the mendicant orders—the Dominicans and the Franciscans in particular—became leaders in the scientific and medical world (though not on the practical level) during the second half of the thirteenth century. Both the scanty scholastic medical literature existing at the time and the limited translation into Latin of Arabic writers on medicine are due to members of these orders.[10] Third, the medical works that would later constitute the doctrinal corpus of thirteenth-century scholastic medicine, a great many of which had been translated into Latin in Toledo during the last thirty years of the twelfth century,[11] were totally absent from the scientific circles of Alfonso X, although in Arabic they circulated within the Jewish and Moorish communities of Castile. A fourth fact, very important in comparison with the translation activity in Castile around 1175, is the reduction in quantity and the decline in scientific quality of the medical contents of the works translated, as can be observed from their astrological character.[12] Fifth, the systematic revision of the legislative corpus that either belongs to or is attributed to the reign of Alfonso X[13] raises the interesting problem of the status of medicine as a profession in the Castile of the second half of the thirteenth century. For at this very time Castile established procedures that made possible social control over professional medical activity and thus joined a process that had been initiated toward the middle of the twelfth century in the Kingdom of Sicily and had spread north during the thirteenth and early fourteenth centuries.[14] Finally, the scientific circles close to Alfonso X, and Alfonso himself, attributed more importance to faith than to science in matters of health and sickness, as if, faced with these problems, natural science could offer no satisfactory answers.[15]

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[8] For the medical translations of Toledo during the twelfth century, see Heinrich Schipperges, *Die Assimilation der Arabischen Medizin durch das Lateinische Mittelalter,* suppl no 3 to *Sudhoff's Archiv* (Wiesbaden Franz Steiner Verlag, 1964)

[9] See Luis García-Ballester, "El papel de las instituciones de consumo y difusión de ciencia médica en la Castilla del siglo XIII el monasterio, la catedral y la universidad," *Dynamis,* 1984, *4* 33–63

[10] Avelino Domínguez García and Luis García-Ballester, "El tratado *de anathomia* (c 1280) de Juan Gil de Zamora (c. 1241–1320)," *Dynamis,* 1983, *3* 341–71 We will return to this topic of limited translations from Arabic into Latin

[11] García-Ballester, *Historia social de la medicina en la España,* 1 31–42, and later in this paper

[12] We refer to the medical content of the following works written at the *Scriptorium* of Alfonso X. *Lapidario,* ed Sagrario Rodríguez M Montalvo (Madrid. Editorial Gredos, 1981), *Libro de las Cruzes,* ed Lloyd A. Kasten and Lawrence B Kiddle (Madrid C S I C, 1961), and *Picatrix,* studied by David Pingree in "Between the *Ghāya* and *Picatrix* I The Spanish version," *J Warburg Courtauld Institutes,* 1981, *44*· 27–56

I shall not attempt in this paper to analyze all these problems in depth, though I will allude to many of them in passing. Instead, I shall dwell on the four elements in thirteenth-century Castile whose contributions (or failure to contribute) had important consequences for medicine: the universities; the cathedrals and monasteries; the Jews, and the mendicant orders.

I

The university is undoubtedly the most important and lasting scientific institution of the late Middle Ages. Throughout Europe from the thirteenth century onward, it developed into the preeminent location for the conduct of the processes of scientific assimilation, elaboration, and transmission. This institution is crucial to our understanding of the development of medical science in the late Middle Ages, and it is a fundamental part of our effort to explain certain sociomedical phenomena, such as the status of the medical profession and its subjugation to social controls.[16]

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[13] *Las siete partidas del rey don Alfonso el Sabio,* 3 vols (Madrid Real Academia de la Historia [hereafter RAH], 1807), *Opúsculos legales del rey don Alfonso el Sabio,* 2 vols (Madrid RAH , 1836), Rafael Muñoz Garrido and Carmen Muñiz Fernandez, *Fuentes legales de la medicina española, siglos XIII-XIX* (Salamanca Universidad de Salamanca, 1969)

[14] See Paul O Kristeller, "The school of Salerno its development and its contribution to the history of learning," *Bull Hist Med*, 1945, *17* 138–94, Vern L Bullough, *The Development of Medicine as a Profession The Contribution of the Medieval University to Modern Medicine* (Basel S Karger, New York. Hafner, 1966), Ronald J Doviak, "The University of Naples and the Study and Practice of Medicine in the Thirteenth and Fourteenth Centuries" (Ph D diss , City University of New York, 1974), Danielle Jacquart, *Le milieu médical en France du XIIe au XVe siècle* (Geneva Librairie Droz, 1981), Luis García-Ballester, "Los origenes de la profesion médica en Cataluña el *collegium* de médicos de Barcelona (1342)," in *Estudios dedicados a Juan Peset Alexandre,* 2 vols (Valencia. Universidad de Valencia, 1982), 1 129–149, Robert S Gottfried, "English medical practitioners, 1340–1530," *Bull Hist Med*, 1984, *58* 164–82, Katharine Park, *Doctors and Medicine in Early Renaissance Florence* (Princeton, New Jersey Princeton University Press, 1985), Luis García-Ballester, Michael R. McVaugh and Agustín Rubio, "Licensing, learning and the control of medical practice in fourteenth-century Valencia" (forthcoming)

[15] I am studying the medical aspect of the *Cantigas* using both the text and the miniatures of MS Escorial T I I

[16] "Thus one of the purposes of the university was to initiate the professional into a set of attitudes and controls, to give him a professional conscience, and to develop a feeling of group solidarity " Vern L Bullough, "Achievement, Professionalization, and the University," in *The Universities in the Late Middle Ages,* ed Jozef Ilsewijn and Jacques Paquet (Louvain, Belgium Leuven University Press, 1978), p 508

During the thirteenth century, faculties of medicine were being formed within the universities around an academic syllabus that would prove to be extremely sensitive to innovations arriving in the form of works by Greek and Arab doctors. These contributions shaped the curriculum, but at the same time they caused important changes in the scientific production of the faculty members.[17] Generally, we can assert that the final years of the thirteenth century and the first decade of the fourteenth marked the high point of medical scholasticism, which was full of such freshness and vigor and which proved to be so efficient at providing answers to the medical problems raised by the ruling minorities of the age that it was highly attractive to non-Christian scientific minorities like the Jews.[18] Now, in spite of the legal documents of Alfonso X's period, it does not seem that there was any medical instruction or even medical scholarship in any of the universities or *studia generales* in the territories covered by the Crown of Castile. It is not surprising that the outstanding medical figure in thirteenth-century Spain, Arnald of Villanova (c. 1240–1311), was not from the Crown of Castile but from the Crown of Aragon; moreover, he was able to receive scholastic medical training at a *studium* (Montpellier) in the Crown of Aragon.[19]

In his classic work *The Universities of Europe in the Middle Ages,* Hastings Rashdall indicates three general characteristics of the thirteenth-century Castilian *studia* or universities: their close relationship with the crown; the strong influence exerted on them by the cathedral chapter; and their economic system of maintenance based on ecclesiastical tithes. In addition, Rashdall notes that each realm had to have its own university.[20] Alfonso X's kingdom (the Crown of Castile) comprised, unified under the person of the king, the former kingdoms of Castile and Leon, as well as extensive new territories in Andalusia and the kingdom of Murcia; these last two came under the sovereignty of the Crown of Castile between 1243 (capitulation of the kingdom of Murcia) and 1248 (conquest of Seville).

In what had formerly been the kingdom of Castile, two universities existed in the thirteenth century, in Palencia and in Valladolid. It may be that medicine was discussed at Palencia in its golden years (1215–20), when a clear connection existed between this *studium* and the cathedral school at Chartres in matters related to the trivium and the quadrivium,[21] and may have extended to *libri naturales* and *medicinalis scientia.* But the *studium* of Palencia had disappeared by 1250, while there is no evidence for the teaching of medicine at Valladolid in this century

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[17] Luke Demaitre, *Doctor Bernard de Gordon Professor and Practitioner* (Toronto Pontifical Institute of Mediaeval Studies, 1980), Nancy G Siraisi, *Taddeo Alderotti and His Pupils Two Generations of Italian Medical Learning* (Princeton, New Jersey Princeton University Press, 1981), Luis García-Ballester, "Arnau de Vilanova (c 1240–1311) y la reforma de los estudios médicos en Montpellier (1309) el Hipócrates latino y la introducción del nuevo Galeno," *Dynamis,* 1982, *2* 97–158

[18] This phenomenon, which I have termed "the reflux of scholasticism" ("Aproximación a la historia social de la medicina bajomedieval en Valencia," *Cuadernos de Historia de la Medicina Española,* 1969, *8* 45–78), is being analyzed from the viewpoint of the Hebrew language by Dolores Ferre in her doctoral thesis on the Hebrew translations of Arnald of Villanova's medical works The first results are to be found in her master's thesis "La traducción hebrea de las 'Parabole medicationis' de Arnau de Vilanova" (Universidad de Granada, 1983)

[19] Michael R. McVaugh, "Arnald of Villanova," in *DSB,* ed Charles C Gillispie, 16 vols (New York: Charles Scribner's Sons, 1970), 1 289–91

[20] Hastings Rashdall, *The Universities of Europe in the Middle Ages,* new edition by Frederick M. Powicke and Alfred B Emden, 3 vols (Oxford At the Clarendon Press, 1936), 2 64–65

The university of Salamanca, in the kingdom of Leon, had had a rather precarious existence from its foundation shortly before 1230 by Alfonso IX. It was revitalized by Ferdinand III (1217–52) and by Alfonso X (1252–84). Indeed, it was by the latter's action in 1254 that, for the first time in either Castile or Leon, a *studium generale* was given an endowment for two chairs for the teaching of medicine. But the endowment that these chairs received was scanty, to say the least—on a par with that given the humble disciplines that constituted Leon's so-called arts faculty, and five times smaller than that given the chairs in law.[22]

We should also note that Alfonso X's obvious personal interest in academic and sociomedical matters cannot be shown to have been translated into established institutions. The king's preoccupation with such matters is clear in the second and seventh *Partidas;*[23] in the second book of *El Espéculo;*[24] and in the famous law of book 4 of the *Fuero Real,* which provides for social control of the medical profession.[25]

But as a whole these texts reveal a lack of coherence—or even a contradiction. In some, university chairs are referred to; in others, no mention of them is made. Some texts seem to presuppose the circulation of scientific and medical ideas, which can only be explained by the existence of academic institutions, while others follow prescholastic patterns typical of the early Middle Ages. Unfortunately, we still have no critical editions of either the *Fuero Real* (whose very existence is proven only for the 1290s) or the second and seventh *Partidas;* for that matter, we do not yet have a full historical analysis of these legal texts.[26]

It is not in the thirteenth, not even in the fourteenth, but in the fifteenth century that we first find convincing evidence of a functioning medical faculty at Salamanca. The first professor in medicine we can identify there is

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[21] Francisco Rico, "La clerecía del mester," *Hispanic Rev*, 1985, *53* 1–23 Rico's article is of great interest since it offers us a clue to understanding the attitudes, which we shall discuss later, of the Castilian Franciscans Pedro Gallego (c. 1200–1267) and Juan Gil of Zamora.

[22] Rashdall, *Universities of Europe,* 2 65–90, Enrique Esperabé Arteaga, *Historia pragmática e interna de la universidad de Salamanca,* 2 vols (Salamanca F Núñez Izquierdo, 1914–17), 1· 22, Vicente de la Fuente, *Historia de la universidades, colegios, y demás establecimientos de enseñanza en España,* 3 vols (Madrid, 1884–89), 1· 295

[23] The titles of these laws are "Who shall be physicians of the king, and what they must do" (*Part* 2, tit. 9, law 10) and "How the physicians or spicemerchant who offers or sells herbs knowingly in order to kill another shall receive the penalty due to murder" (*Part.* 7, tit. 8, law 7)

[24] *Espéculo,* ed Gonzalo Martínez Díez (Avila. Fundación Sánchez Albornoz, 1985), pp 152–53 book 2, tit. 12, law 4

[25] *Fuero Real,* book 4, tit. 16, law 1, in *Opúsculos legales del rey don Alfonso el Sabio,* 2 146

[26] Alfonso García Gallo, "Nuevas observaciones sobre la obra legislativa de Alfonso X," *Anuario de Historia del Derecho Español,* 1976, *46* 609–70

Joan Ruis,[27] teaching in 1406—a century and a half after Alfonso X had decreed "I see fit to order that there shall be ... two professors in medicine" (1254).[28] Significantly, it is in this century, in 1468, that we first have proof of the presence of scientific texts in Salamanca's library:[29] Aristotle's *Libri naturales,* which were already currency in most European universities after 1250 and provided a methodological and biological basis for a science of medicine,[30] and the writings of Hippocrates and Galen, many of which had been translated into Latin at Toledo in the second half of the twelfth century and had become the basis for a profound renovation of scholastic medicine at, for example, Bologna and Montpellier about 1300.[31] The clearest indication of the weakness of thirteenth-century Castilian university institutions is the fact that it seems to have taken nearly two hundred years for the University of Salamanca to attain the levels achieved by, and discover the material characteristics of, scholastic medical institutions elsewhere in Europe. It would seem, in fact, as if the Castile of Alfonso X were indifferent to the upheaval caused in European intellectual circles by the collection of works we have mentioned, even though many of them had come into existence at Castilian Toledo.

II

In addition to the university, two other medieval institutions played an important role in the circulation of medical ideas: the monastery and the cathedral chapters. The latter were closely linked to the universities.

There has not yet been any systematic research into the contents of Castilian monastic libraries from the thirteenth century (or, for that matter, from previous ones) which would enable us to draw definite conclusions, but the little information that is available lets us state quite firmly that the economic stability achieved by the monasteries in this century was accompanied neither by intellectual growth nor by an interest in the new methods of learning and scientific production associated with the universities[32] If we compare the bibliographical resources of two great Castilian monasteries—San Millán de la Cogolla[33] and Silos[34]—with those of the chapters of the most dynamic Castilian cathedrals (e.g., Burgo de Osma and Toledo), our attention is drawn by the intellectual inertia and poverty shown by the Castilian monastic communities, which isolated themselves from the new scholastic science (including medicine) of the thirteenth century.[35] During the thirteenth century, we know only of the interest for astronomy shown by Martín Gil, abbot of the Cistercian monastery of Moreruela (Zamora), to whom Pedro Gallego, bishop of Cartagena, dedicated, between 1260 and 1267, his abbreviated Latin translation of the *Elementa astronomica* of Al-Fārgānī.[36] As we see, Gil's interest was not focused on Latin scholastic science or medicine, but on Arabic astronomy, an interest that was shared by Alfonso X and his court.

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[27] The first proof we have that the two chairs in medicine were actually functioning comes in 1406, when Joan Ruis was professor See Vicente Beltrán de Heredia, *Cartulario de la universidad de Salamanca, 1218–1600* (Salamanca Universidad de Salamanca, 1970), p 659, Salamanca, *Archivo universitario,* book 1647, fols 35–36

[28] Esperabé Arteaga, *Historia de la universidad de Salamanca,* 1 22

[29] At this date appearing for the first time we find Aristotle and Albert the Great's writings on biology and natural philosophy, the medical works of Salerno, Hippocratic and Galenic writings which entered university circles at Paris, Bologna, and Montpellier in the 1280s and the works of medieval doctors like Johannes de Sancto Amando and Arnald of Villanova (Salamanca, *Archivo universitario,* book 1, fols 126, 166v, 187v, and 229, book 3, fol 110v) See also Julio González, *El maestro Juan de Segovia y su biblioteca* (Madrid C S I C, 1944), Ottokar Bonmann, "De testamento librorum johannis de Segovia," *Antonianum,* 1954, *29* 209–16, Guy Beaujouan, *Manuscrits scientifiques médiévaux de l'université de Salamanque et de ses "Colegios Mayores"* (Bordeaux Feret et Fils, 1962), pp 2–3.

[30] James A. Weisheipl, "The Life and Works of St. Albert the Great," in *Albertus Magnus and the Sciences Commemorative Essays 1980,* ed James A. Weisheipl (Toronto Pontifical Institute of Mediaeval Studies, 1980), pp 13–51, David Lindberg, *Roger Bacon's Philosophy of Nature A Critical Edition* (Oxford Oxford University Press, 1983), p xvii

[31] See n 17

Guy Beaujouan remarked that science in the period that concerns us was still carried out in Castile halfway between the Jewish community and the cathedral,[37] and it is indeed in these two places that we find partial answers to the questions being formulated in this paper. I shall not stress here the decisive role played by the cathedral chapters and schools in the cultural and scientific change that took place in Europe during the twelfth century, except to point out that the Spanish cathedrals in Toledo, Leon, Segovia, and the Ebro valley area played an important part in what has been termed with a certain impropriety the cultural and scientific renaissance of Christian Europe in the twelfth century.[38] Did they remain attentive to the world of medical science in the thirteenth century? Or did they close themselves in, thus abandoning this earlier intellectual and scientific leadership? This is something that has not been studied, certainly not for the field of medicine. Now we can offer a kind of answer, at least for the age under discussion, by focusing again upon the book, the vehicle for disseminating medical science in the medieval world.

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[32] Manuel Díaz y Díaz, "Notas de bibliotecas de Castilla en el siglo XIII," in *Livre et lecture en Espagne et en France sous l'Ancien Régime Colloque de la Casa de Velázquez* (Paris Editions A.D P F , 1981)

[33] Rudolf Beer, *Handschriftensatze Spaniens* (Vienna, 1894), p 367

[34] The manuscript, which lists the bibliographical references, is in the Bibliothèque Nationale, Paris, nouv acq lat. 2169, fol 16v See also Léopold Delisle, *Nouvelles mélanges de paléographie et de bibliographie* (Paris, 1880), pp 105–7, Marius Ferotin, *Histoire de l'abbaye de Silos* (Paris, 1897), pp 262–63

[35] García-Ballester, "El papel de las instituciones," pp 36–37

[36] "Frater P[etrus] divina miseracione cartaginensis episcopus fratri Martini egidii (*marg* abbati) de Morelola salutem et pacen " (fol 49rb), in Biblioteca Nacional, Madrid (M), 8918, fols 49rb-56vb (*Incipit* [hereafter *Inc*] Quoniam artificum operantis) It is listed in Manuel de Castro, *Manuscritos franciscanos de la Biblioteca Nacional de Madrid* (Valencia. Ministerio de E y C , 1973) This MS has never been used. The only MS known was a fragment. Biblioteca comunale, Sarnano (S) [Macerata], E 108, fols 46r-49r, encountered by Gaudenzio Melani, "Un frammento inedito della 'Summa astronomica' del Viscovo francescano Pietro Gallego," *Studi Francescani,* 1943, *15* 79–89 Melani did not identify Martinus Egidius (Martín Gil) with the abbot of the monastery of Moreruela (Morelola *M,* Merola *S*) María I Alfonso Antón, *La colonización cisterciense en la meseta del Duero El ejemplo de Moreruela,* 2 vols (Madrid. Universidad Complutense, 1983) Neither of these MSS is in Lynn Thorndike and Pearl Kibre, *A Catalogue of Incipits of Mediaeval Scientific Writings in Latin,* rev ed (Cambridge, Massachusetts Medieval Academy of America, 1963)

[37] "à mi-chemin, si l'on peut dire, entre *l'aljama* et la cathédrale " Beaujouan, *La science en Espagne,* p 21

[38] See Charles H Haskins's classic work *Studies in the History of Mediaeval Science* (Cambridge, Massachusetts Harvard University Press, 1924)

By studying the Castilian cathedral libraries that have been conserved or by gathering information about what they contained, we can begin to evaluate their role in the complex processes of scientific communication and transmission. Unfortunately, here once again we are faced with a paucity of material. Only one trustworthy thirteenth-century inventory remains for what must have been the rich contents of Galician cathedral libraries.[39] I have not found catalogues from the cathedrals of Astorga or Palencia, in Burgos there is nothing for the thirteenth century;[40] what there is for Sigüenza is lacking in medical significance,[41] and the same is true for Cordoba;[42] nor is there anything in Seville or in Cartagena-Murcia. So it is not at all surprising that what we know of the library resources of thirteenth-century Salamanca[43] should not give us the slightest hint of any interest in medical literature, scholastic or prescholastic, despite the formal provision of two chairs in medicine there. Yet this is in keeping with what we have already concluded about the unimportance and perhaps even the absence of medical instruction at Salamanca in this period.

Nevertheless, the material that existed in the libraries of four Castilian cathedrals—Zamora,[44] Oviedo,[45] Burgo de Osma,[46] and Toledo[47]—allows us to identify a number of interesting developments in the transmission of scientific and medical ideas in thirteenth-century Castile, developments that were basically concentrated in the period of Alfonso X's reign. Having studied this material, I have been able to establish the following points:

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[39] It was published by Henri Omont, "Catalogue de la bibliothèque de Bernard II, Archévêque de Saint-Jacques-de-Compostelle (1226)," *Bibliothèque de l'Ecole des Chartres*, 1893, *54* 327–33 There are no medical works, although among the borrowed books (which for that reason are not included in the inventory) is "Totum Avizennemum in tribus corporibus " Bernard II was archbishop of Santiago between 1224 and 1237, it would seem that he studied at Paris The library is rich in books on law and the Scriptures, as well as on the subjects studied at the so-called arts faculties The "Totum Avizennemum" would have fallen into this last category, since it no doubt belonged to the group of Avicenna's works translated into Latin by Domingo Gundisalvo in Toledo in the second half of the twelfth century (whose nucleus is the *Sufficientia* or *Kitāb al-Shifā'*, an encyclopedia in four parts. logic, physics, mathematics, and metaphysics) Avicenna's Aristotelianism dominated the arts faculties until the arrival of the new version by Averroes of Aristotle's writings on natural philosophy See Georges C Anawati, "Ibn Sīnā," in *DSB* (suppl 1), 15 494 On Archbishop Bernard II and his library, see the opinions of Antonio García y García, *La canonística medieval en Galicia* (Santiago de Compostela Universidad de Santiago, 1981), pp 21–25 Unfortunately, owing to lack of chronological data, the work of José Vill-Amil y Castro, "Los códices de las iglesias de Galicia," *Revista de Archivos, Bibliotecas y Museos*, 1873, *3* 283–85, 297–99, 309–11, 328–31, 346–51, 363–67, 370–73, 1874, *4* 141–42, 152–56, 170–72, 218–20 and 348–51, cannot be used for our purposes

[40] The earliest inventories of the Burgos cathedral library belong to the late fourteenth and early fifteenth centuries, and correspond to donations and legacies from canons and bishops Archivo de la catedral, Burgos, MS no 53, fols 619–49, Demetrio Mansilla, *Catálogo de los códices de la catedral de Burgos* (Madrid Instituto Enrique Flórez, 1952), pp 15, 155–73, and 175

[41] José Rius Serra, "Inventario de los manuscritos de la catedral de Siguenza," *Hispania Sacra*, 1950, *3* 431–65

[42] Antonio García y García, Francisco Cantelar Rodríguez and Manuel Nieto Cumplido, *Catálogo de los manuscritos e incunables de la catedral de Córdoba* (Salamanca. Universidad Pontificia de Salamanca, 1976)

[43] Manuel Gómez Moreno, "Inventario de la catedral de Salamanca (año 1275)," *Revista de Archivos, Bibliotecas y Museos*, 1902, 7 175–80, Florencio Marcos Rodríguez, "La antigua biblioteca de la universidad de Salamanca," *Hispania Sacra*, 1961, *14* 281–319

[44] Maria L Guadalupe, "El tesoro del cabildo zamorano aproximación a una biblioteca del siglo XIII," *Studia Historica (Historia Medieval)*, 1983, *1* 167–80

[45] Juan I Ruiz de la Peña, "Alfonso Nicolás, burgués de Oviedo y alcalde del Rey," *Asturiensia Medievalia*, 1975, *2* 113–76

[46] Tomás Rojo Orcajo, "Catálogo descriptivo de los códices que se conservan en la Santa Iglesia Catedral de Burgo de Osma," *Boletín de la Real Academia de la Historia*, 1929, *94* 655–792, *95* 152–314

[47] José M Millás Vallicrosa, *Las traducciones orientales en los manuscritos de la biblioteca de la catedral de Toledo* (Madrid C S I C, 1942)

1. What Beccaria has called "pre-Salernitan medical literature"[48] was still circulating in Castile in the thirteenth century. This literature was composed of a limited legacy from classical Greek medicine, and literature of a lesser order, of poor scientific quality, even including superstitious elements, which was written between the fifth and eleventh centuries. These short works of a practical nature gave advice in the form of remedies using medicinal herbs.[49] This literature had enjoyed a wide diffusion elsewhere in Europe but only up to the eleventh century.

2. In the second half of the thirteenth century, virtually the entire corpus of Salernitan literature arrived in Castile, including both the medical works written before Constantine[50] and the first translations by Constantine and Johannes Afflacius, as well as the elaborations carried out throughout the twelfth century. This corpus contained two different kinds of medical texts. First, a number of general treatises that eventually fused into an academic syllabus—the *Articella*—around which the early teaching of the faculties of medicine revolved until late in the Middle Ages; the core of this syllabus included the *Ysagoge* of Johannitius, the *Aphorismi* and *Pronostica* by Hippocrates, short semiological works on the pulse and urine, and, later, the *Tegni* by Galen.[51] Second, a variety of specialized and solidly elaborated treatises concerning three subjects fundamental to medicine. *dieta* (establishing the foundations of what was to become a rich tradition in the Western world), *pharmacia* (the famous *Antidotaria*), and *chirurgia* (a rich production that would continue to be published in the sixteenth century).[52]

3. For the Castilian cathedral chapters, the raw material of Salernitan medical science was material for use and consumption rather than a basis

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[48] Augusto Beccaria, *I codici di medicina del periodo presalernitano, secoli IX, X, e XI* (Rome. Edizioni di Storia e Letteratura, 1956), p 9

[49] At Zamora cathedral we have identified the *Epistola ad Maecenatem* (*Inc* Libellum quam roganti tibi) See Thorndike and Kibre, *Catalogue of Incipits*, col 817 [hereafter TK] The *Epistola ad Maecenatem*, which was circulated in the medieval world under the name of Hippocrates, appears in all the manuscripts accompanying other herbaria, such as the *De herba vettonica* of Antonio Musa and the Pseudo-Apuleius Beccaria, *I codici di medicina*, pp 143, 144–45, 210, 268–71, 324, 340, 373, 386 In the library of the Bishop of Oviedo, Don Miguel (1289–92), was the *Therapeutica* or *Practica* (*Inc* De alopecia et ophyasy Contingit hec duplex passio .) of Alexander of Tralles (c. 525–c. 605), which was probably translated in Ravenna in the seventh century and forms part of the medical writings which link the classical tradition to the Christian scientific world of the early Middle Ages (TK, 259 and 364) Beccaria, *I codici di medicina*, pp 25, 27, and 409

[50] The most significant of these, also found in the library of the Bishop of Oviedo, is the *Practica* of Petrocellus (*Inc.* Medicinalis tractatus specialiter secundum philosophorum), TK, 859

[51] We have discovered the *Ysagoge* of Johannitius in the cathedral at Burgo de Osma (Rojo Orcajo, "Catálogo descriptivo," pp 658–63), and a collection of the *Articella* (*Ysagoge, Pronostica, Liber pulsuum* of Philaretus and *De urinis* of Theophilus) in the library of Toledo cathedral, MS 97–25, fols 1–52v (Millás Vallicrosa, *Las traducciones orientales*, pp 125–27)

[52] In a thirteenth-century inventory of the cathedral at Burgo de Osma we have found the *Liber urinarum* of Ishāq b Sulaymān al-Isrā'īlī [Isaac Judaeus] (*Inc.* In latinis quidem libris), the *Megategni* of Galen (*Inc* Quoniam intentio gloriosissimi), the *Liber pantegni* of 'Alī b al-'Abbās al-Mağūsī [Haly Abbas], the *Viaticum of Abū Ğa'far Abū b Ibrābīm b Abī Hālid al Ğazzār (Inc.* Quoniam ut ait tallius), and the commentary of

for original scientific creation.[53] There is not the slightest trace in Castile of the scientific and medical production that Salernitan literature generated as it spread through the rest of Europe during the thirteenth century—and no hint, significantly, of any academic assimilation of the *Articella.*[54]

4. The so-called medical corpus of Toledo cannot be found in the Christian scientific institutions of thirteenth-century Castile. This corpus is the complex of medical works both Greek (basically the famous pathology and therapeutics of Galen) and Arabic (at the head of which we find Avicenna's *Canon,* the authentic key to interpretation, on which traditional Galenic medicine was based until the seventeenth century).[55] Its systematic introduction into European universities did not begin until the 1280s, but it was already being used by scholastic authors fifty years earlier.[56] In Castile, however, where the university was not functioning properly in this period, the corpus could not be absorbed.

Yet, if we have managed to detect the raw material of Salernitan medi-

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Matthaeus Olatearius on the *Antidotarium Nicolai* (*Inc* Liber iste quem in presentia[rum] legendum) For the inventory, see Rojo Orcajo, "Catalogo descriptivo," pp 658–63

The thirteenth-century medical manuscripts preserved in the library at Toledo cathedral are nos 98-3 and 98-4 The medical works from Salerno are *Liber dietarum* (*Dieta particulares*) (*Inc* Complevimus in libro primo universalem significationem granis), MS 98-3, fols 4r-28vb, *De stomacho* (*Inc.* Oportet nos intelligere quia prima actio nature), MS 98-4, fols 75r-83r (TK, 1012), *Pantegni,* trans Johannes Afflacius, MS 98-4, fols 46r-59v (TK, 1447), *Antidotarium* (=*Pantegni,* lib X) (*Inc* Antiqui medici bifariam intendunt medicine compositionem in experimentis), MS 98-4, fols 1r-46r (TK, 110), *Liber de modo medendi et opurgandi tribus modis* (*Inc* In medendis corporibus et maxime purgandis variis sepe), MS 98-4, fols 110r-119v (TK, 691), *Liber pauperum, de capillis cadentibus* (*Inc* Capillis ex raritate cadentibus cutis inspissetur), MS 98-4, fols 59v-72r (TK, 186), *Practica magistri Bartolomei Bituriensis* (*Inc* Practica dividitur in duo, in scientiam conservativam sanitatis et curativam egritudinis .), MS 98-3, fols 69v-87va (TK, 1080), *Circa instans,* MS 98-3, fols 88ra-111ra, MS 98-4, fols 90rss, *Rogerina major* (*Inc.* Sicut ab antiquis habemus auctoribus et eorundem .), MS 98-3, fols 29r-65vb (TK, 1479), *De urinis* de Maurus Salernitanus (*Inc* Quoniam de urinarum scientis tractaturi sumus), MS 98-3, fols 118ra-124ra (TK, 591) For the description of the Toledo manuscripts, see Millás Vallicrosa, *Las traducciones orientales,* pp 141–44 and 145–47

[53] The only scholastic activity we have detected at the cathedral school of Toledo during the second half of the thirteenth century was that of Alvaro of Toledo between 1270 and 1300 His work revolved around astrological, astronomical, and cosmological works by Johannes Hispanus, Gerard of Cremona, Alpetragius, Abraham ibn Ezra, and Averroes (*De substantia orbis*) See MSS 47-15 and 98-21 (323-Inv 1727) Manuel Alonso, *Comentario al "De substantia orbis" de Averroes (Aristotelismo y averroismo) por Alvaro de Toledo* (Madrid· C S.I C , 1941), Millás Vallicrosa, *Las traducciones orientales,* pp 64, 156–66 and 180–202

[54] We should remember that the first commentaries at Montpellier on Salernitan literature appear at the end of the first third of the thirteenth century, with the commentaries on the *Ysagoge* by Henry of Winchester (*fl* 1239) and that the definitive introduction of the *Articella* into the curriculum at the University of Montpellier took place in the middle of this century (c 1240) Michael R McVaugh, "The 'Humidum radicale' in thirteenth-century medicine," *Traditio,* 1974, *30* 259–83, and *idem,* "An early discussion of medicinal degrees at Montpellier by Henry of Winchester," *Bull Hist Med,* 1975, *49* 57–71, García-Ballester, "Arnau de Vilanova," pp 99–104

[55] If we widen the scope of our investigation to include the fourteenth-century medical manuscripts that have been preserved at Toledo cathedral, we find two copies of Gerard of Cremona's version of the *Canon* (MSS 47-19 and 47-20) I do not think that their presence in Toledo can be interpreted as proof of a link with the medical tradition of the *corpus toletanum* of the twelfth century Remember that the *Canon* was first used in medical circles at European universities around 1240 (Danielle Jacquart, "La réception du *Canon* d'Avicenne comparaison entre Montpellier et Paris aux XIIIe et XIVe siècles," *110ème Congrès Nationale des Sociétés Savantes,* Sect. d'Hist des Sciences et des Tech , Montpellier, 1–5 April 1985 (in press) I think they are here, not because they had been copied *in situ Toleti,* but, like the rest of the Salernitan literature mentioned, because they were brought and circulated by Castilian clerics educated in Europe Unfortunately, they have not been studied from a codicological point of view, and therefore I cannot give further details

[56] For this process at Montpellier and Paris, see my article "Arnau de Vilanova," pp 109–46, see also Jacquart, "La réception du *Canon.*" For the same process in the north of Italy, see Siraisi, *Taddeo Alderotti and His Pupils*

cine (from the second half of the eleventh and the twelfth centuries), why have we found none of the raw material of Toledan medicine? To what can we attribute Castile's break with and/or ignorance of its own medical tradition?[57] Why was this rich medical corpus from Toledo not assimilated in Castile, when it was so readily transmitted and creatively elaborated in European universities (Montpellier, Bologna, Paris, and so on)? Only at the end of the thirteenth century do we find an indication of the presence of this corpus in Castile—and then as a re-importation from the universities of Italy and northern Europe—in the career of the Franciscan Juan Gil of Zamora (c. 1240–c. 1320). Juan Gil had studied in Paris and traveled across Italy, England, and France and returned to Castile, not to a university setting, but to the court of Alfonso X, where the king named him tutor to his rebel son and successor, Sancho. Gil wrote a medical work on the poisons and projected a scientific encyclopedia that would describe (in alphabetical order) the contents of the microcosm and macrocosm. The extant portion of his encyclopedia shows a good knowledge of the new scientific and medical texts, but as far as we know, the work never went beyond the letter *A;* nor did that fragment arouse any interest in Castile.[58]

III

We have used Guy Beaujouan's image of the situation of science between the Jewish community and the cathedral to help us explore these most fertile territories for the development of scientific and medical material in Castile in the second half of the thirteenth century. We began by looking at the cathedral. We now turn to the Jewish community's use of medical science.

The first thing we must say is that this subject goes beyond the study of the introduction and transmission of scientific ideas produced in Europe.

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[57] I believe that to speak of a medical tradition at Toledo in the twelfth century and those that followed is inappropriate Everything seems to indicate that Toledo was the place where medical works were translated, but that the work of translation was not followed up by one of copying and elaboration European scientists came and went. Christian Toledo was a scientific market rather than a place of medical study

[58] His intention was similar to that of the Dominican Thomas of Cantimpré (c 1204–c 1280) Luis García-Ballester, ed , *De natura rerum (lib IV-XII) por Tomás de Cantimpré Estudio preliminar, transcripción y traducción* , 2 vols (Granada Universidad de Granada, 1974), 1 18–34 The work has been preserved in two manuscripts Staatsbibliothek Preussischer Kulturbesitz, Berlin, lat. 934, fols 62–304 (Valentin Rose, *Verzeichnis der lateinischen Handschriften der koniglichen Bibliotheke zu Berlin,* 2 vols [Berlin, 1901–5], 2(3) 1147–51), and Biblioteca de El Escorial, Madrid, P I 10, fols 1–73 (Guillermo Antolin, *Catálogo de códices latinos de la Real Biblioteca de El Escorial,* 5 vols [Madrid Imprenta helenica, 1910–16], 3 263–64) The letter *A* deals with 167 items in more than 1,200 typed pages Together with Dr Avelino Domínguez García, I am editing this important scientific encyclopedia, which has rich and interesting medical material and has remained unpublished to date Domínguez García and García-Ballester, "El tratado *de anathomia* (c 1280) de Juan Gil de Zamora (c 1241–c 1320), " (n 10) I speak of the little interest in the work in Castile because only two manuscripts have been found. Juan Gil's work on poisons has been edited by Manuel de Castro ("Iohannes Aegidii Zamorensis, OFM, *Contra venena at animalia venenosa.* Introducción, edición y notas," *Archivo Ibero-Americano,* 1976, *36* 3–116) This edition is based on a Vatican MS (*Urbinates latini* 1404, fols 1r-99v) I encountered another manuscript copy of the *Contra venena* in Palma de Mallorca (Fundación Bartolomé March. Biblioteca, MSS s a., 14th c , fols 1r-107r), which is slightly different from that of the Vatican

The Jewish communities in Castile during the thirteenth century were still centers that maintained and produced original medical science. Unfortunately, here again there is a paucity of material; there is, however, sufficient material to allow me to make a few statements that, I believe, will help, if not to explain, at least to complicate a little more the picture presented by medicine in thirteenth-century Castile, and, of course, raise a few more questions.

The Christian reconquest of the twelfth and thirteenth centuries led to the flight of most Muslim scholars (including philosophers and physicians) from Castile, and those Muslims who chose to remain encountered increasingly difficult social conditions. Under these circumstances, the learning and the importance of the Muslim or *Mudejar* doctor in Christian Spain was bound to suffer a decline.[59] This does not mean that Greco-Arabic medical literature disappeared from the territories conquered by Christians during the eleventh, twelfth, and thirteenth centuries, from Madrid-Toledo to Seville-Murcia, for it is still to be found there. We are not referring to the persistence of an Arabism expressed in Latin medical literature, we have already seen that the only Latin Arabism in circulation in thirteenth-century Castile was Salernitan. We are referring to the persistence, in thirteenth-century Christian Castile, of medical literature—both by classical Greek authors, especially Galen, and by Arabs, especially Rhazes—written in Arabic.

For while the scientific language for the Christian scientific community elsewhere in Europe was Latin, in the thirteenth century Arabic was still the undisputed language for science in large areas of the Spanish Christian kingdoms, and was used in the Jewish communities and even by such Christian doctors as Arnald of Villanova, who had received a scholastic university training.[60]

This persistence of Arabic-language scientific and medical literature in thirteenth-century Castile had a number of features that should be stressed: (1) It was not at all medical writing of a secondary nature, that is, short, minor treatises; on the contrary, it included the most important and extensive medical writings of Galen and the classical Arabs. (2) This literature was circulated and used in urban centers—because medical science was an urban phenomenon—basically by members of the Jewish minority, though the Christian Mozarabic and the Muslim or *Mudejar* minorities may well have been involved. (3) It was taught and perpetuated in the midst of these communities and was subjected to a limited degree of scholastic elaboration written in Arabic. (4) There is no evidence that these medical writings in Arabic were used by members of the Christian majority in the second half of the thirteenth century in academic centers like the weak university of Salamanca, or the mixed schools (*studia*) (Arabic-Spanish-Latin-Hebrew) created at this time in Seville and Murcia.[61]

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[59] García-Ballester, *Historia social de la medicina*, 1 29–75, *idem*, *Los moriscos y la medicina un capítulo de la medicina y la ciencia marginadas en la España del siglo XVI* (Barcelona. Editorial Labor, 1984)

[60] See n. 19

By analyzing the resources in Arabic manuscripts from several libraries,[62] I have been able to establish the existence of a wealth of medical literature in Arabic that was being read and copied in various Castilian cities within the Jewish (and perhaps Muslim) community, and outside the university and the Christian majority. The complex of works in circulation is impressive not only for its diversity but also for its importance.

If we organize this material, we find that the Hippocratic corpus is represented, but only by the *Aphorisms* and books from the *Epidemics* and *On Prognostics*;[63] that is to say, by books that were eminently didactic and clinical in character. On the other hand, it includes the most interesting of Galen's extensive medical production, ranging from his treatises on anatomy and physiology (*De usu partium* and *De facultatibus naturalibus*)[64] to his semiological writings (*De symptomatum differentiis, De differentiis febrium*),[65] writings on pathology (*De locis affectis, De crisibus, De diebus criticis, De differentiis morborum*),[66] on therapeutics and dietetics (*De compositione medicamentorum, De alimentorum facultatibus*),[67] and on hygiene (*De sanitate tuenda*).[68] One of the manuscripts contains, grouped together, the collection of Galen's most important medical works (pathological and clinical) which in Latin scholastic medicine would go under the title *De morbo et accidenti*,[69] and would be the nucleus of the important commentaries on general pathology in the freshest and most attractive scholastic writing during the transition from the thirteenth to the fourteenth century.[70] Together with this impressive sample from the Galenic corpus, works of the great masters of Jewish and Arabic medicine were also in circulation; the writings of Rhazes, Avicenna, Haly Abbas, Avenzoar, and Maimonides were copied frequently and assiduously.[71] Many of these writings had not been translated from Arabic into Latin in the Toledo of the end of the twelfth century, and some of them, by both Greeks (Galen) and Arabs (Rhazes,

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[61] Sarton, *Introduction*, 2(2) 834, Beaujouan, *La science en Espagne*, pp 21–22

[62] The most interesting data are to be found in the Arabic manuscripts of the Escorial (Hartwig Derenbourg and H.-P-J Renaud, *Les manuscrits arabes de l'Escurial*, book 2, fascicle 2, *Médecine et Histoire Naturelle* [Paris. Librairie orientaliste Paul Geuthner, 1911]) My conclusions are based on an analysis of the reading and transmission certificates existing in the manuscripts themselves On the usefulness of these certificates, see Georges Vajda, *Les certificats de lecture et de transmission dans les manuscrits arabes de la Bibliothèque Nationale de Paris* (Paris CNRS, 1957)

[63] Derenbourg and Renaud, *Les manuscrits arabes*, p 83 (no 873–8)

[64] *Ibid*, pp 57 (no 850), 54 (no 846)

[65] *Ibid*, pp 12 (no 799-1), 29 (no 818-2), 56 (no 849-3)

[66] *Ibid*, pp 12 (no. 799-2), 56 (no 849-2), 55 (no 849-1), 29 (no. 818-2), 12 (no 799-1)

[67] *Ibid*, pp 14 (no 801-2), 15 (no 802-1)

[68] *Ibid*., p 16 (no 802-3)

[69] *Ibid*., p 91 (no 880)

[70] For the commentaries at Montpellier, see García-Ballester, "Arnau de Vilanova," pp. 131–34, for the commentaries at Bologna, see Siraisi, *Taddeo Alderotti and His Pupils*, p 102

[71] Derenbourg and Renaud, *Les manuscrits arabes*, pp 20–84 (nos 806, 807, 810, 814, 816, 817, 823, 833, 870, 873)

Avenzoar), would not appear in Latin until the 1270s or 1280s, when they contributed in a decisive way to the renovation of scholastic medicine in European university circles[72]—outside of Castile.

The presence and circulation of these works among the Jewish and Muslim communities in thirteenth-century Castile tells us of the vitality and originality of medical science in these minorities, whose knowledge of Arabic guaranteed them access to the most authentic sources of medical science. However, their own medical writings do not yet possess a creative dimension, and tend to be of the nature of medico-scientific *florilegia.* Truly creative elaborations made their appearance among the Jewish community of Castile a century later. They were Arabic manuscripts copied at Toledo in 1376 and at Guadalajara in 1387 using texts of Hippocrates, Galen, Paul Aegineta, Rhazes, Maimonides, ibn Māsawayhi, and others. They were usually in the form of questions and answers, but one extremely important work in essay form has survived.[73] It is interesting to note that the text that was most frequently used to structure this scholastic medical production was the *Ḥāwī* (*Continens*) of Rhazes, and not the *Canon* of Avicenna, the latter a work that was not as well received in Al-Andalus as it was in the Latin Western world.[74] Is this apparent absence of medical creativity in the Jewish communities of the thirteenth century real, or might it be a product of our current very limited knowledge? Scholars today have only a small fraction of these Arabic-Jewish medical writings left to study. The increasing repression and eventual expulsion of the minorities from Spain in the later Middle Ages resulted in the destruction of much of their literature,[75] and cultural factors have continued up to the present to obstruct our knowledge of the portion that has survived.[76]

[72] For example, the Latin version, translated from Arabic, of Galen's work *De interioribus* began to circulate at Montpellier between 1250 and 1300 (Luis García-Ballester and Eustaquio Sánchez Salor, eds , *Arnaldi de Villanova Opera Medica Omnia (AVOMO),* vol 15, *Commentum supra tractatum Galieni de malicia complexionis diverse* [Barcelona Universidad de Barcelona, 1985], pp 31–33, Richard Durling, ed , *AVOMO,* vol 15, *Doctrina Galieni de interioribus,* pp 299–302 The *Continens* of Rhazes was not translated into Latin until 1279 (Sarton, *Introduction,* 2(2) 833), and the Latin version of Averroes' *Colliget* did not begin to circulate until 1285 (MS, Biblioteca Malatestiana, Cesena [Italy], D XXV 4, fol 28r) On the debate concerning the other possible dates for the translation of the *Colliget* proposed by Steinschneider, see Michael R. McVaugh, ed , *AVOMO,* vol 2, *Aphorismi de gradibus* (Granada. Universidad de Barcelona, 1975), pp 61–62

[73] The most important systematic medical treatise written in Arabic by a Castilian Jewish physician was the *Kitāb al-tibb al-qāstalī al-malūkī* (*Book of Regal Castilian Medicine*) It was probably written in the first two decades of the fourteenth century, the author was physician to Ferdinand IV, king of Castile (1295–1312) The work is still unpublished. There are two copies, one was finished in Toledo on 10 February 1414 (Christian era) (Biblioteca Nacional, Madrid, Arabic manuscripts, D C I , fols 1r-38v), the other one was copied from the former in 1782 by Mariani Pizzi and has survived in the British Museum (William Cureton, *Catalogus codicum manuscriptorum Orientalium qui in Museo Britannico asservantur,* 3 vols (London Imprensis curatorum Musei Britannici, 1838–71), vol 2, *Codices arabici,* Supplementum, p 458 Codex DCCCCXXXVI, Add 11,272) We are preparing a critical edition with Spanish translation and commentaries in collaboration with Professor Concepción Vázquez (University of Salamanca) and Professor Pedro Gil (University of Navarra at Pamplona).

[74] Roger Arnaldez and Albert Z. Iskandar, "Ibn Rushd [Averroes]," in *DSB,* 12 7b[*sic*]-8 See Derenbourg and Renaud, *Les manuscrits arabes,* pp 82–84 (nos 873–8 and 873–11)

[75] García-Ballester, *Historia social de la medicina,* pp 50–53 and 59–64

[76] As an example we could cite the interesting Muslim-Spanish manuscripts which have survived in the library of the al-Qārawīyīn at Fez (Morocco) and have not yet been catalogued For socioreligious reasons these manuscripts were not moved away to Rabat, the capital.

IV

In the field of medicine, therefore, there was a double omission in Castile in the relationship between the Christian Latin world and the Arabic-speaking minorities. In the first place, the translation program from Arabic into Latin or from Arabic into Castilian in thirteenth-century Castile (and in the reign of Alfonso X in particular) did not include any of the medical works that were circulating in Arabic among the Jewish and Muslim minorities in the cities. In the second place, Castilian scientists with Latin scholastic training received in European university circles (men who held ecclesiastical positions in Zamora, Santiago, Salamanca, Cordoba, Burgo de Osma, Sigüenza, and Toledo, as well as some doctors at the court), together with the Castilian university institutions themselves, chose to ignore the flourishing medical knowledge in the Jewish communities of their own cities, knowledge that was used by Jewish or Muslim doctors with whom they must have shared their daily lives.

The only exception to this double omission is to be found in the kingdom of Murcia, which became part of the kingdom of Castile after being conquered from the Moors in 1251–55.[77] There, members of the mendicant orders (Dominicans and Franciscans) showed enough curiosity about Arabic medical science to maintain a small program of translation and study. This program appears to have been limited and localized, yet it is very interesting, for it reveals to us the survival of a pattern of relations between the Latin and Arab scientific and cultural worlds; it suggests that it may have been possible, at the end of the thirteenth century, to renew the process of transmission of medical works from Arabic into Latin in the territories in the south of Castile. These territories were unique in bordering territories of Arab culture (the Muslim kingdom of Granada, North Africa)—which, in fact, was still the dominant culture among the population of the newly won lands—as well as the Mediterranean Sea (which meant that intense commercial and cultural relations were quickly established with northern and southern Italy after the Christian conquest).[78]

The first translations from Arabic to Latin in these territories were made by the Franciscan Pedro Gallego (c. 1200–1267), who was named first bishop of Cartagena (Murcia) in 1250.[79] During the seventeen years of his bishopric he completed translations of four works with implications for medicine: an abridgement (with personal touches) of the *Liber de animalibus* (*Historia animalium,* books 1–9) of Aristotle,[80] an abridgement of

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[77] Julio González, *Reinado y diplomas de Fernando III,* vol 1, *Estudio* (Cordoba Publicaciones del Monte de Piedad y Caja de Ahorros de Córdoba, 1980), pp 340–54

[78] Juan Torres Fontes, "El obispado de Cartagena en el siglo XIII," *Hispania,* 1953, *13* 339–580

[79] A. Pelzer, "Un traducteur inconnu Pierre Gallego, franciscain et premier evêque de Carthagène (1250–1267)," in Franz Ehrle, *Scritti di storia e paleografia miscellanea Francesco Ehrle,* 5 vols (Rome Biblioteca Apostolica Vaticana, 1924), 1 408–56, Atanasio López, "Fr Pedro Gallego, primer obispo de Cartagena (1250–1267)," *Archivo Ibero-Americano,* 1925, *12* 65–91

[80] Vatican, *Vat lat* MS 1288, fols 131r–155r Georgius Lacombe, *Aristoteles latinus* Codices Pars prior (Rome La libreria dello Stato, 1939), p 226, Pelzer, "Un traducteur inconnu," pp. 435–44

Averroës' compendium to the *De partibus animalium* (*Historia animalium,* books 1–2, chapter 2) of Aristotle,[81] and a text on *Oeconomia* (*in regitiva domus*), probably based on the pseudo-Galenic work.[82] He also wrote a *Summa astronomica,* in which he appears to summarize the treatise of Al-Fārgānī, *Elementa astronomica.*[83] We do not know if he had carried out any scientific activity before his arrival in Murcia, but we have to remember that he had a strong friendship with Alfonso X and belonged in the court of the king before his appointment as bishop of Cartagena;[84] it is also perfectly possible that the peculiar socioeconomic and socioscientific situation of Murcia provided him with a scientifically stimulating atmosphere.

The second place in Murcia where medical works were translated was the school of Arabic and Hebrew languages (*studium arabicum et hebraicum*) founded by the Dominicans in 1266 for missionary and apologetical purposes. This school was similar to others created in the thirteenth century in the territories on the borders of Christendom ("Germania," "Hungria," "Hispania").[85] For reasons unknown to us, Rufino Alejandrino (from Alessandria in Italy, to the north of Genoa) arrived in Murcia to learn Arabic. He was taught the language there by a Dominican, Dominiscus Marrochinus.[86] With the latter's help he translated at least two works: the first was an ophthalmic work, which he finished in 1271;[87] the second was a group of works collected in a manual like those used in teaching medicine and entitled *Liber quaestionum medicinalium discentibus in medicina,* attributed to "Huneyn filius Ysaac medici qui latine dicitur Johannicius."[88]

This second item is an authentic student textbook, very similar to the *Articella* elaborated in the Latin Christian world of Salerno.[89] It consists of three clearly differentiated parts (*particule*): the first (*theorica seu sciencia*) has a question-and-answer format and largely coincides with the original Arabic of the *Isagoge* of Ḥunain b. Ishāq;[90] the second part (*practica*) is devoted to remedies and concludes with a chapter on the pulse (*de pulsu*);[91] and the third part, after a brief chapter on ages (*de etatibus*), is composed of brief writings (*de exercicio, de febribus, de apostema, de urina*).[92] This type of work, with simple contents and a schematic or erotetic (question-and-answer) presentation, often aroused greater interest among Christian, Jewish, and Arab doctors in the late Middle Ages than the extensive and complex works of the Greek and Arab masters. That this Arabic-language students' manual should have been current in Murcia between the years 1270 and 1280 is of great interest, and tells us of the presence and circulation among the Muslim and Christian population of a medical literature elaborated and prepared for didactic purpose, in addition to telling us of the existence of another Latin version of the *Isagoge* and "another" *Articella* overlooked by scholars.

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[81] Vatican, *Vat lat* MS 1288, fols 159r–161r Lacombe, *Aristoteles latinus,* p 227, Pelzer, "Un traducteur inconnu," pp 446–47.

[82] Vatican, *Barb lat.* MS 52, fols 22r–24r, Bibliothèque Nationale, Paris, *lat* MS 6818, fols 28r-30v Pelzer, "Un traducteur inconnu," pp 423–28

[83] See n 36 Professor Julio Samsó of the University of Barcelona is studying the work.

[84] López, "Fr Pedro Gallego," pp 68–71

[85] Ugo Monneret de Villard, *Lo studio dell'Islam in Europa nel XII e nel XIII sècolo* (Vatican City Biblioteca Vaticana, 1944), pp 35–37, José M. Coll, "Escuelas de lenguas orientales en los siglos XIII y XIV," *Analecta Sacra Tarraconensia,* 1944, *17* 132–35, Angel Cortabarría Beitia, "San Ramón de Penyafort y las escuelas dominicanas de lenguas," *Escritos del Vedat,* 1977, *7* 125–54.

[86] Thomas Kaeppelli, *Scriptores Ordinis Praedicatorum Medii Aevi* (Rome: S Sabinae, 1970), vol. 1 (A–F), pp 328–29

[87] "Explicit libri de egritudinibus oculorum [Iesu filii Haly] translatus Murcie per magistrum Dominicum fratrum predicatorum rogatu magistri Rufini de Alexandria scolaris eius in arabico et sociorum, gracias del alto et vero. Anno domini MCCLXXI." Burgerbibliothek, Bern, 216, fols 1ra–43vb This copy dates from 1279 (fol. 43vb)

[88] "Explicit Johannicius translatus a Rufino Alexandrino cum adiutorio magistri sui in arabico fratris Dominici Marrochini de oridine fratrum predicatorum in Murcia de arabico ad latinum Deo gracias" Staatsbibliothek Amploniana, Erfurt, F 271, fols 11ra–25rb In the manuscript (fol 18vb) there is a mention of "Huneyn filius Ysaac medici qui latine dicitur Johannicius" The first news of this work was given by Moritz Steinschneider, *Die hebraeischen Ubersetzungen des Mittelalters und die Juden als Dolmetscher* (Berlin, 1893, reprint, Graz Akademische-u Verlagsenstalt, 1956), pp 710–11.

[89] Paul O. Kristeller, "Bartholomaeus, Musandinus, and Maurus of Salerno and other commentators of the 'Articella,' with a tentative list of texts and manuscripts," *Italia Medioevale e Umanistica,* 1976, *19* 57–87

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> Medicine, as the learned Ancients showed, means both the knowledge to recognise things as they are according to nature and to know the influence of each thing on the others; thus those who possess this skill in abundance can do much good and cure many ills, especially by looking after men's lives and health, ridding them of illnesses in which they suffer great injury or come close to death. And those who do this are called physicians, who not only fight to cure men's illnesses but also keep them in health so that they may not become ill.[93]

This text from the second *Partida* makes it clear that the Castile of Alfonso X accepted the importance of medicine as a social activity and of physicians as a peculiarly skilled group. What emerges from the rest of the scanty information available to us, as I have tried to show, is that this acceptance does not seem to have entailed an interest in the new medical science becoming available in the thirteenth century. In Christian Castile, neither the institutions that had traditionally been the means of preserving and communicating knowledge in the earlier Middle Ages—the monasteries and the cathedrals—nor the emerging universities that would take their place, give any evidence of paying attention to the new translations of Greco-Arabic texts that had been produced in their very midst, in twelfth-century Toledo. While Castilian Jews (and perhaps Muslims) assimilated this scientific literature, there remained a gulf between their medicine and that of the Christian community. Christian Castile finally became acquainted with

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[90] Erfurt, F 271, fols 11ra–13vb

[91] *Ibid.*, fols 13vb–20rb

[92] *Ibid*, fols 20rb–vb (*de etatibus*), 20vb–21va (*de exercicio*), 21va–23rb (*de febribus*), 23rb–va (*de apostemate*), 23va–25rb (*de urina*)

[93] *Part* 2, tit. 9, law 10

Salernitan medicine in the early thirteenth century, and went no further for a long time.

These circumstances pose a difficult question for future historians to answer. Castile possessed cultures in which sophisticated medical learning was very much alive; it was the source from which most of that new learning made its way, in Latin, to the rest of Christian Europe; it had a monarch apparently supportive of scientific inquiry; and it shared in the new university movement of the thirteenth century. It would seem, therefore, to have been even more progressive in these ways than its neighboring kingdom in the Iberian peninsula, the Crown of Aragon—yet Aragon shows plainly the powerful effect of the new knowledge, and Castile does not.

What lines of interpretation may eventually prove to be useful in explaining this anomaly? It may be that the mood of the *reconquista* in Castile was harsher, less willing to profit from a pagan culture than was the case in Aragon; or that Castile had a generally less developed urban society, in which the new medicine could flourish; or that the personal interests of Alfonso X were fixed upon astronomy to the detriment of medicine, while those of Jaime II of Aragon (1291–1327) were markedly in the new scholastic medicine; or that Castilian universities were too weak to be able to sustain the new learning. Other factors could be suggested. But at the moment we are still too poorly informed to be able to decide which, if any, of these factors played a part. All we can do for now is acknowledge the paradox that Christian Castile, which gave so much Greco-Arabic medical learning to the Latin West, showed no interest in making use of it.

EXPERIMENTAL PHYSIOLOGY, MEDICAL REFORM, AND THE POLITICS OF EDUCATION AT THE UNIVERSITY OF HEIDELBERG: A CASE STUDY*

Arleen Tuchman

Although modern scientific methods had little to offer medical practice before the end of the nineteenth century, physicians throughout Germany began to rally together in the 1840s to fight for major changes in medical education and licensing.[1] As part of their reform demands they advocated increasing the amount of time spent in studying the natural sciences, more practical clinical experience, and improved laboratory training. In addition, they fought to unify the profession and abolish the traditional division of medical personnel into three classes: physician, surgeon, and obstetrician. In the 1850s physicians began achieving considerable success. In 1852 Prussia abolished the tripartite examination system, establishing a single license for all medical practitioners. Baden followed six years later. Moreover, in these years, Baden, Bavaria, Prussia, and Saxony instituted stricter regulations aimed at controlling the quality and content of the university medical curriculum. As of 1858, for example, Baden required of all medical students not only clinical experience, but laboratory work, as well, and three years later Prussia followed suit.[2] These measures, accompanied by stricter licensing requirements, reflected a general interest in raising standards among the future physicians of the state.

If, however, the "first goal of medicine is to heal,"[3] then the adoption of methods and techniques that contributed very little to medical practice needs explaining. No doubt the advances made by the experimental

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[1] Alfons Fischer, *Geschichte des deutschen Gesundheitswesens,* 2 vols (1933, reprint, Hildesheim Georg Olms, 1965), 2. 301

[2] Baden's new medical regulations were reprinted in the local medical journal, *Mittheilungen des badischen ärztlichen Vereins* (hereafter *Mitt bad. arztl Ver*), 1858, *12*(*3*) 17–20 In Prussia similar reforms in the state medical examination did not occur until 1861 See Klaus Sczibilanski, "Von der Prufungs- und Vorprufungsordnung (1883) bis zur Approbationsordnung 1970 für Arzte der Bundesrepublik Deutschland. Die Entwicklung der medizinischen Prufungsordnungen, darg an Aufsatzen aus der deutschen arztlichen Standespress" (Ph.D diss , Universitat Munster, 1977), pp 12–14

[3] Robert Volz, "Versammlung deutscher Naturforscher und Arzte," *Mitt bad. àrztl Ver ,* 1858, *12*(*8*) 140 Later in the paper I discuss Volz's recommendations concerning the experimental method.

sciences in the understanding of bodily functions influenced the eagerness with which physicians fought for medical reform, but their interest in these new methods also involved socioeconomic and professional factors that extended beyond these immediate cognitive developments.

I shall argue that the decision to reform medical education in the 1850s reflected a growing appreciation of the power of scientific and technical knowledge by a society slowly undergoing the transition from an agricultural economy to a modern industrial state. In the 1850s Germany was well into its so-called first phase of industrialization, a period when the railroad, telegraph, credit banks, and new forms of production were beginning significantly to restructure German society[4] It was within this context that the experimental sciences found a home in the German university system, and the construction of large research laboratories began.[5] And for physicians, it was an education in the experimental sciences, and especially in experimental physiology, that promised to provide them with a part in this transformation.

This article deals with these developments through a case study of the various factors involved in the institutionalization of experimental physiology at the University of Heidelberg in Baden, Germany. To set the broader political background, I provide a description of the government's science policy between 1830 and 1850 before moving on to the consequences of this policy for the kind of education offered to the future physicians of the state. Were medical interests primarily in the foreground? If so, what did experimental physiology have to offer medicine in the 1850s? Finally, I turn to the interest of physicians in helping to bring about a reform of medical education.

State interest in promoting an education in the sciences and technology increased in the 1830s and 1840s. In those years governments throughout Germany began to reform the education system.[6] In the southern German state of Baden, government ministers worked together with the parliament to create trade schools for artisans and secondary schools that focused on the sciences and modern languages for the slowly emerging commercial class. In addition, they made natural history, natural philosophy, and mathematics required courses in the classical *Gymnasia,* they increased the budget for the state polytechnical school from twenty thousand to thirty-four thousand gulden per year; they built a new anatomy and zoology institute at the University of Heidelberg; and they began plans for an all-inclusive natural science institute that not only would serve university purposes but would be open to the public, as well.[7]

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[4] Friedrich-Wilhelm Henning, *Die Industrialisierung in Deutschland, 1800 bis 1914* (Paderborn Ferdinand Schoningh, 1973)

[5] According to conventional wisdom, the rapid success of the experimental sciences in Germany was due in large degree to the decentralized structure of the university system. As Joseph Ben-David and Avraham Zloczower have shown, the nineteen universities in Germany proper were in stiff competition with one another, creating an academic market that permitted professors to set conditions for their appointments See Joseph Ben-David, "Scientific productivity and academic organization in nineteenth-century medicine," *Amer Soc. Rev,* 1960, *25*(*2*) 828–43, Avraham Zloczower, "Career Opportunities and the Growth of Scientific Discovery in Nineteenth-Century Germany, with Special Reference to Physiology" (Ph D diss , Hebrew University, 1960) While the importance of this competitive structure is not to be denied, the state, as we will see below, was by no means merely reacting to the dynamics of the academic market, it had definite interests in promoting the experimental sciences

[6] See Peter Lundgreen, *Sozialgeschichte der deutschen Schule im Uberblick, vol 1, 1770–1918* (Gottingen Vandenhoeck & Ruprecht, 1980), Friedrich Paulsen, *Geschichte des gelehrten Unterrichts auf den deutschen Schulen und Universitaten von Ausgang des Mittelalters bis zur Gegenwart,* 2 vols (Leipzig Veit, 1896–97), Walter Scholer, *Geschichte des naturwissenschaftlichen Unterrichts im 17. bis 19 Jahrhundert* (Berlin Walter de Gruyter, 1970)

The parliamentary representatives made no secret about the reasons for these reforms. With one eye focused on the advanced state of the English economy, and the other on Baden's increasing population and poverty, the representatives argued that an education in those skills necessary for improving the economy was of utmost importance.[8] They were not, however, merely concerned with creating a forum for the transmission of scientific and technical information; rather, they were seeking to educate a citizenry capable of, and interested in, developing, improving, and promoting new means of production. What they sought was a replacement for a degenerate guild system that they believed had proven to be more a hindrance than a help for production. True to Adam Smith's writings, they rejected the idea of state involvement in business enterprises or the use of prohibitive measures or high tariffs for the promotion of manufacture and trade. Rather, they believed that improvements in the means of production depended upon the existence of an open system based on the free flow of valuable ideas. "Knowledge," argued Ludwig Winter, president of the State Ministry, "knows no guilds, . . . only freedom."[9] Within this context, laboratory training in the exact method of the natural sciences took on particular importance. According to Carl Nebenius, minister of education, this method taught the student "to see with his own eyes, examine, and contemplate." By offering the opportunity for thorough instruction, he added, the government

> finally places [the members of] this class [of producers] in the position to make their own proper observations at the workplace, reflect on what they have

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[7] Arleen Tuchman, "From the Lecture to the Laboratory. The Institutionalization of Scientific Medicine at the University of Heidelberg," in *The Investigative Enterprise,* ed Frederic L. Holmes and William Coleman (Berkeley and Los Angeles University of California Press), in press

Until 1858, 1 gulden equaled 0 67 taler or the equivalent of 2 01 marks After 1858, 1 gulden equaled 0 56 taler or the equivalent of 1 68 marks See Frank R. Pfetsch, *Zur Entwicklung der Wissenschaftspolitik in Deutschland, 1750–1914* (Berlin Duncker und Humblot, 1974), p 45

[8] See, for example, the remarks of Carl F Nebenius in the proceedings of the lower chamber of the Baden parliament. Baden, Standeversammlung, 2 Kammer, *Verhandlung der Stande-Versammlung des Grossherzogthums Baden,* 137th sess , 17 Nov 1831, Protokollheft 30, pp 234–41 (The proceedings will hereafter be referred to as *Verh der Stande-Versammlung*) Ludwig Winter in *Verh der Stande-Versammlung,* pp 262–63 See also Franz Schnabel, *Deutsche Geschichte im neunzehnten Jahrhundert,* 2d ed., 4 vols (Freiburg im Breisgau Herder, 1950), vol. 3, *Erfahrungswissenschaften und Technik,* pp 300–301

[9] Cited in Loyd E Lee, *The Politics of Harmony Civil Service, Liberalism, and Social Reform in Baden, 1800–1850* (Newark University of Delaware Press, 1980), p 115

> observed as well as on phenomena accidentally noticed, and perform well-directed experiments, discovering thereby new truths that can find immediate profitable application in the area of production.[10]

The appreciation of the advantages to be derived from an improved education in the sciences took on a new dimension in the years following the revolution of 1848. As Peter Borscheid has shown, the government attributed the recent uprisings less to dissatisfaction with the political system than to the hunger and poverty that had resulted from the agricultural disasters of the 1840s.[11] Previously indifferent to Justus Liebig's campaign to use modern chemical methods for increasing the productivity of the land, the government began in the 1850s to invest extravagant sums in experimental chemistry. It helped finance a new chemistry laboratory at the polytechnical school, developed a new educational program for the agricultural "sciences," and sponsored several lecture series in chemistry for small landowners, peasant farmers, and industrialists.[12] Its greatest investment, however, was the appointment of Robert Bunsen at the University of Heidelberg in 1852, and the construction of a new chemistry institute with ample student laboratory space.[13] Completed in 1855, the laboratory cost seventy thousand gulden and was one of the first major research institutes in all of Germany. Support for the other experimental sciences followed soon after. In 1854 the physicist Gustav Kirchhoff was hired by the University of Heidelberg,[14] and it was during the 1850s that the government expressed an interest in experimental physiology. A reflection of a general commitment to an education in the natural sciences, this interest was also motivated by a conviction that medicine would thereby benefit. What experimental chemistry was doing for agriculture, experimental physiology would do for medicine, namely, provide a basis for a scientific method of procedure.

In 1852 Jacob Henle, who had been teaching anatomy and physiology at Heidelberg for almost a decade, decided to accept an offer from the University of Göttingen.[15] There he was to have responsibility for anatomy alone. He had spent the previous three years trying to convince Heidelberg to hire a young professor to take over his courses in physiology and allow him to concentrate on anatomy, but this had led nowhere. Although the administration had shown an interest in fulfilling this wish, negotiations with potential candidates had dragged on so long that Henle had grown skeptical of the government's good intentions. Worried, moreover, about the flourishing conservative political atmosphere in the aftermath of the revolution of 1848—Henle had actively supported moderate liberal reforms—he took advantage of the offer from the University of Göttingen to improve his situation, both politically and professionally.

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[10] Carl F Nebenius, *Uber technische Lehranstalten in ihrem Zusammenhange mit dem gesammten Unterrichtswesen und mit besonderer Rucksicht auf die polytechnische Schule zu Karlsruhe* (Karlsruhe, 1833), p 52 (This passage and others quoted in the paper are translated by the author unless otherwise indicated) This passage was cited during the parliamentary meetings See the article by the parliamentary representative Kroll, "Commissionsbericht uber die Errichtung von höhern Burger- und Gewerbsschulen," *Verb der Stande-Versammlung*, 66th sess , 24 Sept 1833, Beilagenheft 5, pp 252–54

[11] Peter Borscheid, *Naturwissenschaft, Staat und Industrie in Baden* (*1848–1914*) (Stuttgart Klett, 1976)

[12] *Ibid*, pp 28–57 Also Anon , "Einrichtung einer landwirthschaftlicher Schule zu Karlsruhe," in *Karlsruher Zeitung*, 17 Sept 1851

[13] Die Lehrkanzel der Chemie, so wie die Stelle eines Directors des chemischen Laboratoriums, 1836–1923, File 235/3133, Badisches Generallandesarchiv Karlsruhe [Baden State Archives], Karlsruhe, Federal Republic of Germany (hereafter Badisches Archiv) Also Borscheid, *Naturwissenschaft, Staat und Industrie*, pp 60–65

[14] For the negotiations surrounding Kirchhoff's appointment, see Die Lehrstelle der Physik und deren Besetzung, 1816–1930, File 235/3135, Badisches Archiv

[15] The events surrounding Henle's decision to leave Heidelberg are discussed in Hans-Heinz Eulner and Hermann Hoepke, eds , *Der Briefwechsel zwischen Rudolph Wagner und Jacob Henle, 1838–1862* (Gottingen Vandenhoeck & Ruprecht, 1979)

In the discussions surrounding Henle's replacement, the focal questions remained whether to hold the position intact or whether to hire two professors, one for anatomy and the other for physiology. The Baden Health Commission took an active interest in this discussion. Organized in 1803, the commission consisted primarily of physicians and had, as part of its responsibilities, control over the state medical examination and the granting of licenses. Moreover, it had a say (albeit only in an advisory capacity) in the kind of education and training offered to medical students.[16] In the months just prior to Henle's departure, the commission had already recommended to the Ministry of Interior that the two subjects be separated:

> To stay in the spirit of the times it is necessary to focus on cultivating the exact method in true scientific research and to train students more in the method of examining and utilizing natural objects This being so, . [a] teacher, acquainted with the exact method and capable of heading a physiological institute with success, must be called.[17]

The Health Commission was echoing a general concern of physicians with the kind of education being offered to medical students. In Baden, state-employed physicians had formed a local medical organization in 1835, private physicians in 1844, and both had consistently placed great importance on the natural sciences as a way to raise standards in the profession and improve physicians' social standing in society. In their local journals, physicians pleaded with their colleagues to end the pettiness, bickering, and competition that divided the profession, hurt its image, and weakened its collective strength. Instead, they argued, physicians must share their skills and experience, uniting around the common goal of promoting scientific knowledge. In this way medical care would improve, and physicians would form a united front capable of protecting their own interests.[18]

In their specific reform demands, physicians placed great emphasis upon the importance of the experimental sciences. No longer considering Latin and Greek to be appropriate for training the mental faculties of would-be doctors and natural scientists, physicians turned to the exact method of the experimental sciences; this would be the new tool for helping students learn to think clearly and critically. But what was meant by the "exact" method? In the broadest sense it signified the search for laws of nature of such a general character that no exceptions to the rule would exist. Such laws would enable scientists to make definite predictions of future events, and it was this certainty of prediction, rather than the mathematical description of natural events, that gave an "exactness" to the method of the experimental sciences.[19] This, in fact, was the criterion used by Hermann Helmholtz to generate a hierarchy of the various branches of knowledge. In a speech delivered in 1862 on the occasion of his election to the position of rector at the University of Heidelberg, Helmholtz gave expression to ideas that had been actively defended in Baden for well over a decade. The more general the law—that is, the more phenomena for which it could account—he explained, the more sophisticated the science.[20] He placed the humanities, consequently, at the lower end of the scale, the experimental sciences and mathematics at the top. In this speech, moreover, he elaborated upon the benefits gained from the derivation of such universal laws: not only a deepening of our understanding of organic processes, but an increase in our ability "to exploit the powers of nature for our benefit and make them subservient to our will."[21]

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[16] For information on the Health Commission, see Die Sanitats–Commission in objectiver Hinsicht, File 235/27692, Badisches Archiv Also Karl Stiefel, *Baden, 1648–1952,* 2d ed , 2 vols (Karlsruhe Verein für oberrhein Rechts- und Verwaltungsgeschichte, 1979), 1 1284

[17] Health Commission to the Ministry of Interior, 23 June 1852, File 235/3133, Badisches Archiv

[18] The main instrument used by the Baden medical society in fighting for its reform demands was the medical journal *Mittheilungen des badischen arztlichen Vereins,* founded by the society in 1847 (the editor was Robert Volz) On the formation of medical societies in Germany, see Fischer, *Geschichte des deutschen Gesundheitswesens,* 2· 368–87

A greater understanding of processes was valuable because it allowed control, manipulation, and, ultimately, change. "Knowledge is power," Helmholtz argued—power that, in the 1840s and 1850s, was already beginning to make itself felt in the application of steam, in weaving and spinning machines, in transportation and communication.[22] And although Helmholtz valued all forms of knowledge—whether chemical, physical, legal, or economic—he gave most credit to the experimental sciences for transforming knowledge directly into power.

As we have already seen, the Health Commission had, in June 1852, recommended hiring an experimental physiologist. Now, a few months after Henle's departure, the Ministry of Interior wrote to the State Ministry with its own assessment of the situation. Expressing particular concern with the sharp fall in enrollment in the medical faculty (the number of students had dropped from 165 to 94 within five years),[23] the Ministry of Interior agreed with the Health Commission that two separate appointments should be made:

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[19] The deterministic implications of the demand in the 1840s for a scientific medicine have been discussed recently by William Coleman in "Experimental Physiology and Statistical Inference The Therapeutic Trial in Nineteenth-Century Germany," in *The Probabilistic Revolution*, ed Lorenz Kruger et al , 2 vols (Cambridge, Massachusetts. M.I T Press, 1987), 2 201–26

[20] Hermann Helmholtz, "Uber das Verhaltnis der Naturwissenschaften zur Gesammtheit der Wissenschaft," Akademische Festrede gehalten zu Heidelberg am 22 Nov 1862 bei Antritt des Prorektorats, in Hermann von Helmholtz, *Vorträge und Reden*, 5th ed , 2 vols (Braunschweig Vieweg, 1903), 1 169

[21] *Ibid*, p 177

[22] *Ibid*, pp 180–81

> We believe that there must be one Ordinarius for anatomy and one Ordinarius for physiology, and that the anatomical and physiological institutes must be separated from one another. .. Both positions should be filled with excellent talents; for physiology, namely, a man must be won who is devoted to the exact method. Because of the direction which the entire course of medical studies has taken in our day, it is our conviction that physiology must be taught in its entirety and in a totally satisfactory fashion at a university that wants to enjoy high repute and attendance.[24]

This early attempt by the government to hire an experimental physiologist failed, however, because of opposition voiced by the medical faculty. Although the professors acknowledged the importance of the experimental direction in physiology, they resisted the appointment of one of the young experimentalists of the so-called 1847 group, namely Carl Ludwig, Hermann Helmholtz, Emil DuBois-Reymond, and Ernst Brücke.[25] Unfortunately, they did not specify the reasons for their opposition, but one can safely speculate that personal and political motivations were strong contributing factors. The senior professors Maximilian Josef Chelius and Friedrich Puchelt had been in Heidelberg for more than thirty years and had acquired much influence in the Baden government.[26] Indeed, a previous accusation by Henle that the older faculty members had tried to establish dynasties can be supported by appointment records: in 1835–36 the son of the obstetrics professor Franz Karl Nägeli began lecturing at the university, being promoted to assistant professor in 1839; in 1841 Puchelt's son also began teaching; and in 1847 Chelius's son followed suit, being promoted to assistant professor in 1852.[27] Moreover, the members of the faculty formed a politically conservative group, and the liberal views of Ludwig and his cohorts may not have been to their liking. The person they did promote for the chair of physiology, Friedrich Arnold, was a man of their own scientific style and political conviction.[28] Although the Ministry of Interior expressed concern that physiology would not be taught "in the necessary way," it showed little interest in opposing the faculty.[29] In July of 1852 Arnold was hired; according to his nephew Wilhelm Wundt, Arnold "belonged ... to an older generation for whom physiology was still essentially a subject subsidiary to anatomy.... Arnold lacked the physicalist training necessary for the establishment of an independent physiological science."[30]

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[23] Enrollment statistics were printed every year in *Grossherzoglich Badisches Regierungsblatt* (Karlsruhe). Falling enrollments at Heidelberg did not reflect a general trend in medical schools at the time. On the contrary, enrollments at other universities increased steadily until 1854, after which they reached a plateau until the end of the decade. See Franz Eulenberg, *Die Frequenz der deutschen Universitäten von ihrer Gründung bis zur Gegenwart* (Leipzig: Teubner, 1904), p. 255.

[24] Ministry of Interior to the State Ministry, 23 July 1852, Die Lehrstellen der medicinischen Fakultat, Der Lehrstuhl für Pathologie, Therapie und die Direktion der medicinischen Klinik, File 235/3141, Badisches Archiv.

I wish to point out that it was the Ministry of Interior, backed by the Health Commission, that fought for the creation of a separate chair and laboratory for experimental physiology in 1852. These demands did not come directly from the physiologists themselves. In fact, in 1855, Carl Ludwig accepted a position in Vienna for physiology and zoology, and in the same year Helmholtz went to Bonn as professor of anatomy and physiology. This is not to say that the young experimentalists did not fight for the establishment of their own discipline, but in the early 1850s they were not yet in a position to make concrete demands. Baden's interest in supporting this new science was not, therefore, a response to conditions being set by potential candidates, but rather a reflection of the government's general commitment to the experimental sciences which characterized its educational policies in the 1850s.

[25] Medical faculty to the Ministry of Interior, 26 July 1852, File 235/3141, Badisches Archiv.

[26] Henle to Wagner, 21 July 1852, in Eulner and Hoepke, eds., *Der Briefwechsel zwischen Rudolph Wagner und Jacob Henle*, pp. 62–63.

[27] All promotions are listed in *Anzeige der Vorlesungen auf der Grossherzoglich badischen Ruprecht Karolinischen Universitat zu Heidelberg*.

In 1856 there were personnel changes in the government, and the new president of the Ministry of Interior, Franz Freiherr von Stengel, returned to the issue of separating the anatomical and physiological subjects.[31] This was not von Stengel's first encounter with university politics. In the 1840s he had played a major part in modernizing the medical faculty, being largely responsible for the creation of two new positions and the appointments of Jacob Henle and Karl Pfeufer.[32] Now, in 1856, he was faced once again with the task of reforming the medical faculty. The question was not whether Arnold should retain responsibility for anatomy, but whether an entirely new position should be created for a professor of physiology. Von Stengel turned to the Health Commission to request its opinion on the matter, and he received a detailed report in return.

"What was physiology," asked Franz Bils, director of the Health Commission and author of the report, "when it was still being taught by anatomists, pathologists, etc.? And what has it become since it has acquired its own teachers?" The question was not merely rhetorical; Bils proceeded to describe physiology's transition from a speculative body of knowledge to a modern science, a transition made possible by advances in chemistry, physics, microscopy, and mechanics. By following in the footsteps of these sciences, physiology had "itself become a natural science, that is, in the sense of an exact science." He then added,

> Here is a general answer to the Ministry of Interior's question. Namely, physiology can be taught only by someone who is familiar with the exact sciences. The anatomist, even if he is very highly educated in his subject, is still engaged in a descriptive science, and this does not qualify him as a teacher of physiology In physiology things must be explained, description is not enough[33]

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[28] For information on Arnold, see Gurlt, "Friedrich Arnold," in *Biographisches Lexikon*, 5 vols (Munich and Berlin Urban & Schwarzenberg, 1962), 1 210–11 Also Arleen Tuchman, "Science, Medicine and the State The Institutionalization of Scientific Medicine at the University of Heidelberg" (Ph D diss , University of Wisconsin, 1985), chap 6

[29] Ministry of Interior to the State Ministry, 23 July 1852, File 235/3141, Badisches Archiv

[30] Wilhelm Wundt, *Erlebtes und Erkanntes* (Stuttgart Kroner, 1920), pp 73–74

[31] For information on von Stengel, see "Franz Freiherr von Stengel," in *Badische Biographien*, ed Friedrich von Weech, 6 vols in 4 (Heidelberg C Winter, 1875–1935), 2 311–13

[32] Tuchman, "Science, Medicine and the State," chap 3

[33] Health Commission to the Ministry of Interior, 10 Dec. 1856, Die Lehrstellen der medicinischen Fakultat. Der Lehrstuhl für Physiologie, File 235/29872, Badisches Archiv

A reader who wanted or even expected specific information on the advantages to medicine resulting from this transformation in physiology would be disappointed Bils merely claimed that this science had grown more practical. He may have omitted specifics because therapeutics had, in truth, benefited very little from scientific research.[34] Nevertheless, the reader does not have the impression that Bils was discouraged by the absence of tangible gains. Perhaps this was because diagnostics had not fared as poorly as therapeutics We have only to think of the use of chemical analysis, as in the identification of Bright's disease, as well as the invention of the stethoscope, spirometer, kymograph, and ophthalmoscope.[35] By 1850, these developments had already begun to bring a certain measure of objectivity to the diagnosis of disease, although the interpretation of the information they uncovered often remained unclear. Bils's optimism, however, seems to have been motivated primarily by recent successes in physics, chemistry, and physiology, and he believed that medicine, by adopting the same experimental method, would also benefit. In a fashion reminiscent of Nebenius's support of laboratory instruction, Bils explained that "by the pursuit of a physiological problem, the young physician learns the method of investigation, which is as well the exact method of medical investigation."[36] That is, when the physician begins to approach medical problems with the same critical eye as the scientist who tackles a problem in the laboratory, a medical "science" will prevail.

The Health Commission was not alone in its high estimation of the exact method. Robert Bunsen, from whom the Ministry of Interior had also requested a recommendation, argued that it was

> not only desirable but also absolutely necessary to consider only those physiology professors who follow the new exact direction. This seems all the more imperative because this new direction, far from representing the specific views of a specific school, distinguishes itself much more from the older [schools] in that it does not simply take the fundamental principles of physiology from other natural sciences, but attempts instead to discover them for itself through the more critical method of independent experimental and mathematical investigations.[37]

Bunsen knew of only four men who fit the bill. Helmholtz, DuBois-Reymond, Ludwig, and Brücke.

Six days after Bunsen sent his report to the Ministry of Interior, Chelius, then dean of the medical faculty, submitted his recommendation to the faculty senate. From the tone of the letter it is evident that the faculty had accepted the inevitability of hiring a physiologist. Nevertheless, Chelius emphasized the success with which Arnold had taught experimental physiology during the preceding years, explaining that Arnold "could not, in the interest of science and education, hand physiology over to a competitor who lacked the competence to represent the subject in its entirety in a dignified fashion."[38] Consequently, the number of potential candidates was limited. Here, as in Bunsen's letter, the four members of the 1847 group took center stage, but Chelius was more selective, sharply criticizing DuBois-Reymond for his overspecialization. The person he favored was Helmholtz, whom he held to be the most capable of establishing a link between experimental physiology and medicine. Chelius had, in fact, reason to believe this. Although it may seem strange today to think of Helmholtz in a medical light, he did actually have considerable medical experience. In exchange for a scholarship that had permitted him to study medicine, he had spent six years working as an army surgeon in Prussia. Far more important for his medical reputation, however, was his invention of the ophthalmoscope in 1850. Even Helmholtz referred to this invention later in life as the most important discovery for his career.[39]

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[34] Erwin H Ackerknecht, *Therapeutics from the Primitives to the 20th Century* (New York Hafner Press, 1973), chaps 10 and 11, Paul Diepgen, *Geschichte der Medizin*, 3 vols (Berlin Walter de Gruyter, 1949–55), 1(1) 163–66

[35] Stanley Joel Reiser, *Medicine and the Reign of Technology* (Cambridge, London, and New York Cambridge University Press, 1978)

[36] Health Commission to the Ministry of Interior, 10 Dec 1856, File 235/29872, Badisches Archiv

[37] Bunsen to the Ministry of Interior, 28 May 1857, in *ibid*

The Ministry of Interior, some members of the faculty, and the Health Commission were not alone in voicing an interest in the hiring of an experimental physiologist. As I have already mentioned, the Health Commission's report reflected a general concern among academically trained physicians with the kind of education being provided at the university. In their local medical organization, physicians concentrated their efforts on promoting collegiality and fighting for the improvement and codification of educational requirements. Examination and licensing regulations had undergone only minor changes since the beginning of the century. The regulations set the length of medical studies at three and a half years, yet a student had absolute freedom to study what he wanted. To qualify for the state medical examination, he had only to fulfill the ambiguous requirement of having acquired "a thorough knowledge of the natural sciences and medicine."[40]

Throughout the 1840s and 1850s physicians sought to tighten these requirements, placing particular emphasis on the importance of practical work in the clinic and in the laboratory.[41] If there were individuals who questioned the value of the latter, they did not make their voices heard. On the contrary, the reigning sentiment was that laboratory exercises provided an essential prerequisite for later clinical work. During such work the student learned, under somewhat simplified conditions, to analyze phenomena in an exact and scientific fashion. Once learned, it was believed, the method could be directly applied to the study of the phenomena of disease. "The clinic," wrote a physician in Baden's local medical journal,

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[38] Chelius to the faculty senate, 5 June 1857, in *ibid*

[39] Helmholtz's comment is in "Erinnerungen (1891)," in Helmholtz, *Vortrage und Reden,* 1 12 For biographical information on Helmholtz, see Leo Konigsberger, *Hermann von Helmholtz,* 3 vols (Braunschweig Vieweg, 1902–3)

[40] *Grossherzoglich Badisches Regierungsblatt* (Karlsruhe), 5 Aug 1828 See also C A. Diez, *Zusammenstellung der gegenwartig geltenden Gesetze, Verordnungen, Instructionen und Entscheidungen uber das Medicinalwesen und die Stellung und die Verrichtungen der Medicinalbeamten und Sanitatsdiener im Grossherzogthum Baden* (Karlsruhe, 1857)

[41] See, for example, the following reports and articles in *Mitt bad arztl Ver* "Durlacher Bezirksverein," 1848, *2(11)* 81–86, "Hohe zweite Kammer des badischen Landestande," 1848, *2(24)* 183–84, "Unsere Aufgabe im neuen Jahr," 1849, *3(1)*. 1–8, "Wie sollen die Arzte gebildet werden," 1852, *6(9)* 65–69

> is by no means the first and only place for learning and practicing the fundamental principles of exact investigation By spending time at the dissecting table, in the chemical laboratory, in the physiological institute—that has not, unfortunately, been actualized—, and through constant practice, the student must become familiar with the only method possible in the natural sciences if he is to learn and know how to investigate facts in an exact way, and how to deduce scientifically legitimate conclusions therefrom[42]

Having learned this method, the author continued, the student would be prepared to enter the clinic, where he would only need instruction in "the correct way to guide this experience through the great labyrinth of pathology and therapy"[43] The mental skills acquired in the laboratory, in other words, would be directly applicable to the study and cure of diseases in the clinic.

In accordance with this belief in the pedagogical value of laboratory experience for later clinical work, the medical society petitioned the government to divide the state examination into two parts: a preliminary examination in the natural sciences and a final one in the medical subjects proper. Moreover, it advocated strongly the abolishment of the classification of physician, surgeon, and obstetrician, favoring a single examination and license for all three subject areas.[44]

The motivations of physicians in calling for these reforms were complex. Surely the physicians were guided by a firm conviction in the benefits to be gained from medical "science" following the model of the natural sciences. Yet professional motivations were also strong. The financial condition and social standing of German physicians at mid-century were deplorable, and physicians believed competition both from non-licensed practitioners and from less qualified colleagues to be a central problem.[45] In the 1840s there were, in fact, nine categories of health practitioners in Baden, each with different skills and educational backgrounds, and although the medical responsibilities of each group were carefully specified, in practice boundaries were often crossed and competition for patients occurred.[46] Some physicians advocated government restrictions of non-licensed practitioners as a means of countering the competition, but many others argued that such measures would be of little use if physicians themselves did not begin to offer better medical care.[47] The demands for stricter educational requirements and a single license thus in part reflected an attempt to raise standards within the profession so as to legitimate the claim to a monopoly over medical care.

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[42] "Wie sollen die Ärzte gebildet werden," *Mitt bad arztl Ver*, 1852, 6(*9*) 66

[43] *Ibid*

[44] "Arztlicher Bezirksverein in Kraichgau," *Mitt bad arztl Ver*, 1848, *2*(*22*) 165–67

[45] Claudia Huerkampf, "Arzte und Professionalisierung in Deutschland Uberlegungen zum Wandel des Arztberufs im 19 Jahrhundert," *Geschichte und Gesellschaft*, 1980, *6* 349–82

[46] Fischer, *Geschichte des deutschen Gesundheitswesens*, 2 371

In 1858 the Baden government decided to create a new chair for physiology and to hire Helmholtz Moveover, it set up a temporary laboratory in a rented building and began immediately to draw up plans for a new research institute for experimental physiology and physics.[48] This was a victory for those interested in reforming medical education, and it was not the only victory. In the same year the government also completely reorganized examination and licensing requirements, establishing a single examination and single license for all physicians.[49] In addition, to qualify for the examination, students had to prove that they had spent one semester each in the physiology and chemistry laboratories, and one year in the medical, surgical, and obstetrical clinics. Practical work, not only in the clinic, but also in the laboratory, had become an established part of the medical curriculum.

Robert Volz, a member of the Baden Health Commission, presented the opening address at the national meeting of German scientists and physicians in 1858. After singing the praises of modern scientific medicine, he reminded his audience of the discrepancy that still existed between theory and practice. The patient, he warned, could not afford to wait until the natural law describing his disease was found, and he would be "just as grateful to the physician for his recovery even if this did not follow an exact scientific course."[50]

Volz's recommendation was not to question the potential usefulness of the experimental method. Without it, the modern physician could hardly aspire to be a scientist. Rather, he cautioned physicians not to expect miracles, while at the same time encouraging them to continue analyzing and measuring:

> Even if the widest field lies fruitless before us, and much has still to be sowed, our task has nevertheless become clear to us, the path to reach it is known It is the path that builds on laws, the path that traces the effects of electrical and magnetic forces back to a general measure, that determines the path of light and measures the size of its waves to the millionth part of a millimeter. But to attain such results, the physicist had to [take] a body (or a symptom) property by property, observing, examining, comparing, experimentally provoking, measur-

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[47] *Ibid*, pp 408–13

[48] Der Neubau für die naturwissenschaftlichen Institute der Universitat Heidelberg, Friedrichsbau, File 235/352, Badisches Archiv For the negotiations surrounding the creation of a chair for experimental physiology and Helmholtz's appointment, see Files 235/3141, 235/29872, Badisches Archiv

[49] "Prufungsordnung für die Kandidaten der Heilkunde," *Mitt bad arztl Ver*, 1858, *12*(*3*) 17–20

[50] The speech is in "Versammlung deutscher Naturforscher und Ärzte zu Karlsruhe," *Mitt bad arztl Ver*, 1858, *12*(*8*) 137–41 The quotation is from page 140 of the text

> ing, and weighing it in order to find ultimately the common laws which its expressions obey
>
> We are also working on this task, and its preliminary and more modest part devolves upon the medicine of our day. We must observe, weigh, and examine But our time does not yet suffice for drawing conclusions Our medical research is still that of facts, that of our sons shall be of causes, of laws. In this way, substance will become motion, and physics physiology. Still, future physicians must always remember that the first goal of medicine is to heal[51]

Through the adoption of the exact method of the natural sciences, modern medicine had sought to become, and in many instances had become, a science. The new medical regulations in Baden and elsewhere gave institutional legitimacy to this transition by guaranteeing that future physicians would be educated in scientific methods and techniques This had occurred in spite of the lack of direct therapeutic gains, but in a society slowly undergoing industrialization, facility with instruments and mastery of experimental techniques—in short, knowledge of an exact and precise nature—had become highly regarded traits. The power of this knowledge was already beginning to make itself felt in the modernization of agricultural techniques, transportation, and communication. This, of course, was only a beginning. A closer link between science and industry, as evidenced in the emergence of chemical-, pharmaceutical-, and electrical-based industries, would not occur until the end of the century. But by mid-century a strong faith already existed that scientific knowledge would provide both the ideological and the material backbone for the emergence of a modern industrial state. And it was this faith that convinced physicians they had only to continue dissecting, weighing, and measuring to guarantee that medicine would some day improve as well.

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[51] *Ibid*, pp 140–41

PUBLIC HEALTH AND THE PRODUCTION AND USE OF DIPHTHERIA ANTITOXIN IN PHILADELPHIA

Jonathan M. Liebenau

Diphtheria antitoxin, the first biological drug to be produced by scientific medicine, dramatically demonstrated the possibilities of a bacteriological understanding of disease. The drug also provided a means by which public health establishments could utilize new laboratory techniques and new treatments on a large scale.[1] This essay describes the introduction of diphtheria antitoxin to Philadelphia, one of the first places it was used in the United States. In the decade at the turn of the century, Philadelphia had a particularly strong medical community, with an active public health body and a rapidly growing pharmaceutical industry. The developments following the transfer of antitoxin from Europe, its production by public health authorities, and the relationship of these officials with the commercial sector are the focus of the paper.

During the decade beginning in 1885, the annual number of deaths slowly fell in Philadelphia, while the population grew. However, the death rate still seemed unacceptably high. Consumption (or pulmonary tuberculosis) remained the major cause of mortality, causing 2,834 deaths in a total population of approximately 930,000 in 1886, and 2,513 in a population of more than 1,600,000 in 1894.[2] Childhood diseases seemed to be a growing problem. "Cholera infantum" (childhood diarrhea) and diphtheria vied with each other as the major cause of death among children, and trailed only consumption and heart diseases as principal causes overall.[3] Diphtheria had been one of the major causes of morbidity and mortality in Philadelphia throughout the century; it was usually responsible for about 5 percent of all deaths and ranked third among causes of death after infancy.[4]

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[1] The biologicals were fundamentally different from earlier drugs both in their manner of production and in the research underlying them In addition, although the techniques of administration—subcutaneous, intravenous, or intramuscular—of biologicals were similar to those of other injected medicines, treatment programs using antitoxins were necessarily different See Henry James Parish, *A History of Immunization* (Edinburgh E & S Livingstone, 1965), pp 118–40; Frederick W Andrewes et al, *Diphtheria Its Bacteriology, Pathology, and Immunology* (London HMSO, 1923), pp 126–29

[2] Philadelphia Bureau of Health, *Annual Report,* 1918 (Philadelphia, 1919), p 151 After 1899, the Board of Health became the Bureau of Health All *Annual Reports* are from the Philadelphia Board (or Bureau) of Health unless otherwise noted

The structure of public health activities in Philadelphia centered on the Board of Health, directed by its trustees The trustees in 1895 consisted of Mayor Charles F Warwick and the director of public safety (who served as ex-officio heads), William H Ford, M D., presided, and A.A. Hirst served as secretary Other members of the board were Albert H. Dingee, Peter D Keyser, and J Louis Good Through standing committees on which they all served, these men supervised seven divisions, including the Municipal Hospital (Philadelphia General Hospital, also known as Blockley), the division of contagious diseases, the milk inspection division, and the corps of twenty-three part-time vaccine physicians

[3] *Annual Reports,* 1880–1920

Diphtheria is a contagious disease that causes a thick membrane to form over the infected person's throat while simultaneously weakening the victim through high fever and exhaustion.[5] Late in the century it was still commonly confused with other throat conditions, although it had been identified earlier in the century as a single, specific disease.[6] The disease could be fatal to adults, but it was more often fatal to children.[7] There were many appalling tales of parents watching their small children slowly die of suffocation from a persistent membrane, or bleed to death from their efforts to clear their throats.[8] Diphtheria lingered in densely populated areas and affected large percentages of the youth in most eastern U.S. cities.[9] When the disease was far advanced, there was no effective treatment. Treatment in the nineteenth century ranged from exorcising demons[10] to removing the membrane by cutting and tearing.[11] Where diphtheria was diagnosed, patients were often put under quarantine, but few hospitals had wards specifically set aside for diphtheria patients until the mid 1890s.[12]

Diphtheria appeared to be an endemic disease, and its history was marked by mysterious fluctuations that seemingly could only be charted, not affected. Some doctors and public health officials, moreover, were becoming concerned with a long-term trend indicating a steadily increasing incidence of the disease.[13] In 1868 there had been 110 diphtheria deaths in Philadelphia, but the number of deaths rose through the 1870s, with epidemic years in 1875 and 1876, during which 652 and 706 diphtheria deaths were reported.[14] Diphtheria continued to claim between 300 and 450 lives until 1883, when deaths doubled, deaths due to diphtheria increased by another third the following year. As inexplicable as the sudden increase in deaths was a sudden halving of the number in 1885. After that the number of deaths fluctuated until diphtheria again became epidemic in 1891 and, though fluctuating somewhat over the following years, remained epidemic through the mid-1890s. The total number of people infected by diphtheria in 1895 was 3,853, of whom more than one-third (1,349) died, almost all of them children.[15] Diphtheria deaths tended to be concentrated in certain sections of the city; for example, there were seventy-eight fatalities in one ward of the city, and only six in a ward across town. The disease accounted for approximately 5 percent of all deaths in most wards, though the figure was almost 10 percent in some areas.[16] Philadelphia's Board of Health attributed the prevalence of the disease in certain areas to the social class of the population, the mode of living and habits of the residents, the character of their dwellings, and other moral and environmental factors.[17]

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[4] *Ibid*, 1895, p. 186

[5] Andrewes et al, *Diphtheria*, pp 13–63.

[6] *Ibid*, Pierre Fidèle Bretonneau, *Des Inflammations Spéciales du tissu muqueux, et en particular de la diphthérite, ou inflammation pelliculaire* (Paris Crevot, 1826)

[7] *Annual Reports*, 1880–1920, William H Park and W L Beebe, "Diphtheria and pseudo diphtheria, a report to Hermann M. Biggs on the bacteriological examination of 5,611 cases ," *Med Record*, 1894, *46* 385

[8] For a graphic description of the effects of diphtheria on children, see "Sir Charles Sherrington's first use of diphtheria antitoxin made in England," *Notes Records Roy Soc London*, 1948, *5* 156–59, also Sinclair Lewis, *Arrowsmith* (London. Jonathan Cape, 1925), Wade W Oliver, *The Man Who Lived for Tomorrow A Biography of William Hallock Park* (New York. Dutton, 1941), p 85 For a description written in the period, see Edwin O Jordan, *A Text-Book of General Bacteriology* (Philadelphia. W B Saunders, 1908), pp 229–31

[9] United States Bureau of the Census, *Historical Statistics of the United States, from Colonial Times to 1970* (Washington, D C Government Printing Office, 1976), p 58, New York Board of Health, *Annual Reports*, 1890–1930, Philadelphia Bureau of Health, *Annual Reports*, 1890–1930

[10] Wayland Hand, "Folk Medicine" (Paper presented at the Smithsonian Institution, March 1980)

[11] Laryngotomy and intubation were commonly used for more severe cases See Edwin Rosenthal, "Reduced period of intubation by the serum treatment of laryngeal diphtheria," *Trans Med Soc State Pennsylvania*, 1896, *27* 238–50, and *idem*, "A report on one hundred cases of diphtheria of the larynx treated by intubation," *Med. Bull*, September and October 1894

[12] The diphtheria ward at Blockley was opened in 1893 See William M Welch, "The Municipal Hospital for Contagious and Infectious Diseases," in *Founder's Week Memorial Volume*, ed Frederick P Henry (Philadelphia City of Philadelphia, 1909), pp 546–54, also Alexander C Abbott, "The Development of Public Health Work in Philadelphia," in the same volume, pp 563–92 See also Charles K. Mills, "The Philadelphia Almshouse and the Philadelphia Hospital from 1854 to 1908," in *History of Blockley A History of the Philadelphia General Hospital from Its Inception, 1731–1928*, comp John Welsh Croskey (Philadelphia F A. Davis, 1929), p 94

[13] *Annual Report*, 1895, p 186

Traditional sanitary measures were carefully enforced by Philadelphia's Board of Health.[18] In conjunction with other interests in the city government, the board succeeded in getting streets paved and having proper drainage installed.[19] The waste removal service for much of the city was improved, and through "constant surveillance of all parts of the City, particularly in the summer time, by the police force, acting as supplementary sanitary inspectors," breaches of sanitary law were identified.[20] The program of sanitary improvements, which progressed steadily during the early 1890s, was carried out in an interventionist style of public health and prepared the city to commence large-scale drug production and purchasing. The nuisance inspector had increased powers to control littering of the streets and could, for example, demand the quick removal of offal from the steps of an offending butcher.[21] Foremost among the goals of sanitarians was the effective collection, analysis, and publication of health records.[22]

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[14] *Ibid*, 1918, p 151

[15] *Ibid*, p 151

[16] Diphtheria is analyzed by ward in *ibid*, 1890 (attached chart), and *ibid*, 1909, pp 86–89

[17] *Ibid*, 1890, p 61

[18] A major activity of the public health administrator was the collection of morbidity statistics Despite the administrators' efforts to prevent disease through environmental control, morbidity and mortality statistics generally indicated no evidence of improvement. In the case of diphtheria, the persistence of the disease despite prompt quarantine, disinfection, and extensive reporting was particularlv frustrating

[19] *Annual Reports*, 1890–1920 See "Annual Report of the Director" in each volume

[20] *Ibid*, 1895, pp 31–32 This attitude was also expressed in the Board of Health Minutes, 1895, Philadelphia City Archives, City Hall Annex.

[21] Board Minutes, see in particular discussions on the role of the nuisance inspector in 1894

[22] In 1894, the compulsory registration of tuberculosis was linked to disinfection The following year, the normal procedure was reportedly as follows "After death from consumption or removal of the sick person, when the Board of Health was so notified, the premises were visited by the Medical Inspector and disinfection ordered, and when not performed by the parties themselves was ordered conducted by the disinfecting corps connected with the Bureau " *Annual Report*, 1895, pp 32, 140

John Shaw Billings of the University of Pennsylvania was very influential in this campaign. He held that sanitationist ideas from earlier in the century needed updating, but he was reluctant to rely entirely on analyses and techniques from laboratory medicine[23] His central concepts represented the enlightened views of the early 1880s. Billings advocated an eclectic transitionary concept of the germ theory which implied the necessity for cleanliness.[24] Placing too much stress on the causative connection between bacteria and specific diseases, he believed, would threaten the primacy of general hygienic measures.[25]

The Board of Health in Philadelphia, as elsewhere, had been dominated by administrators whose main concerns were to maintain the Municipal Hospital (Philadelphia General Hospital) and enforce nuisance and quarantine laws. Billings's endorsement of more active sanitationism led to changes in the priorities of the board, which shifted the blame for disease from the poor and the ill to the rich and politically powerful.

Despite the measures recommended by Billings, which included prompt quarantine and disinfection, diphtheria and other diseases persisted. As a result, the highly touted claims of bacteriologists who hoped to be able to trace and eventually eliminate disease grew in appeal. Since Billings was not officially attached to the board, his position as advisor was vulnerable. Furthermore, Dr. William H. Ford, president of the Philadelphia Board of Health, shared responsibility with the Pennsylvania Board of Health's powerful director, Benjamin Lee.[26] This divided command allowed the bacteriologists some leverage as they tried to move the boards away from an environmental point of view. The first step was to try to establish a laboratory in which bacteriological testing could be undertaken.

The departure in 1894 of John Shaw Billings from the Laboratory of Hygiene at the University of Pennsylvania, and Alexander C. Abbott's subsequent accession to the directorship of that laboratory,[27] strengthened the bacteriologists' position there. Because of the close connection between the laboratory and the Philadelphia Board of Health, the change also helped the bacteriologists in their attempts to gain the acceptance of their subject at the Board of Health.[28] Within the city Board of Health, the attitude toward an independent laboratory was split. The proponents had to fight widespread suspicion about the significance of a correlation between germs and every case of infective disease. This suspicion was fueled by concern that an interpretation of epidemic disease based on a narrow germ theory would undermine the campaign for a broad-based sanitary movement. The traditional correlation between diphtheria and poor living conditions, diet, predisposition, and morality was threatened by a narrow emphasis on the causative role of the Klebs-Loeffler bacillus. Furthermore, it was feared that public health activities might take business away from physicians.

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[23] Fielding H Garrison, *John Shaw Billings A Memoir* (New York and London G P Putnam's Sons, 1915), p 279

[24] *Ibid*, pp 279–80

[25] Billings believed that these were best exercised by such public health activities as waste disposal and disinfection and should be advanced by public education He also stressed the importance of statistics in charting the general effect of public health measures In word, at least, the Philadelphia Bureau of Health followed his environmental approach, paying little attention to the insurgent proponents of bacteriology, who were championed by Billings's assistant, Abbott. Billings's stress on hygiene was enough of a challenge for the more traditional public health workers See Barbara Gutmann Rosenkrantz, ***Public Health and the State** Changing Views in Massachusetts, 1842–1936* (Cambridge, Massachusetts Harvard University Press, 1972), p 98. Billings prepared many morbidity and mortality reports for the U S Bureau of the Census See, e g, Billings, "Vital Statistics of the Jews in the United States," Bulletin no 19 (Washington, D C U S Census Office, 1890)

[26] State of Pennsylvania, *Annual Report of the State Board of Health,* 1894

[27] University of Pennsylvania, catalogues, 1894, 1895, University of Pennsylvania Archives, Philadelphia, Joseph McFarland, "The beginning of bacteriology in Philadelphia recollections of what took place before the beginning of the present century," *Bull Hist Med*, 1937, *5*· 149–98 See also George Washington Corner, *Two Centuries of Medicine A History of the School of Medicine, University of Pennsylvania* (Philadelphia. J B Lippincott, 1965), p 182

The bacteriologists' attack on microorganisms seemed to imply that less moral responsibility for disease would be given to the general populace, while public health workers would be given even more active responsibilities. As Abbott and other bacteriologists gradually defeated the sanitationists, the bacteriologists' ideas seemed to call for a reassessment of the board's methods, which would increasingly involve monitoring disease through bacteriological diagnosing and, ultimately, intervening through therapeutic and prophylactic means. This intrusive form of public health justified both mass vaccination and later, the systematic antitoxin treatment. The first priority, though, was the production of diphtheria antitoxin.

THE TRANSFER OF ANTITOXIN TO THE UNITED STATES FROM EUROPE

Philadelphia's efforts to produce diphtheria antitoxin followed pioneering work at the New York City Department of Public Health. While both the New York and the Philadelphia attempts began in the autumn of 1894, the greater influence of the bacteriologists in New York, and their ability to garner funds and immediate support, put them far ahead[29]

As in Philadelphia, there had been longstanding concern in New York with the incidence of diphtheria. However, bacteriologically oriented physicians had control of the New York laboratory, established in 1892 for diagnostic purposes, and had been able to build that facility into a leading laboratory of its kind, with the enthusiastic support of the public health commissioner and the city government.[30] Moreover, New York's ties with German and French scientists at this critical time were even closer than those enjoyed by Philadelphia. New York and Philadelphia were rival port cities, of almost equal size But the immigrants of the late nineteenth century arrived in the port of New York in much greater numbers, posing a significant public health threat.[31] In searching for new and more effective ways to detect epidemic diseases, especially cholera, in immigrants, laboratory science was given an early chance.

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[28] Board Minutes, 1894 As Rosenkrantz states, "The social obligation of responsible citizens was increased rather than diminished by contrasts between affluence and poverty, by evidence that good will was an inadequate stimulus for reform Scientific and social laws converged, and, when properly understood and utilized, could compensate for the frailty of errant individuals" (*Public Health*, p. 96)

[29] Oliver, *Man Who Lived for Tomorrow*, pp 106–10. The New York laboratory raised supplementary funds through a public appeal

[30] *Ibid*

Some Americans, thus, were well prepared for the transfer of diphtheria antitoxin to the United States, although they remained dependent on Europe for their fundamental biological education. Visitors to Europe, led by Joseph J. Kinyoun of the U.S. Marine Hospital Service and Hermann M. Biggs of the New York City Department of Health,[32] carried back the new information.

Kinyoun had long been interested in German bacteriological work, having studied in Europe. He had established a diagnostic laboratory in the Service's hospital on Staten Island in 1887.[33] His prime charge was to conduct pathological analyses, but his work soon included the growth of bacterial cultures. The influence of the German school was particularly apparent in the equipment this laboratory used.

> The different apparatus supplied was modelled after those used in the laboratory of Dr. Koch, of the Imperial German Health board, and is supplied with Zeiss' latest improved microscope objectives and microphotographic apparatus
>
> Different animals are kept on hand for experimental purposes Additions have been made from time to time, until a very complete plant of apparatus exists for analysis and bacteriological investigations. In October 1887 experimental studies were made from cases of Asiatic cholera occurring among the emigrant passengers on the steamship *Alesia,* followed later by the examination and diagnosis made upon cases occurring on the steamship *Britannia,* demonstrating the existence of Asiatic cholera, which was subsequently confirmed by other investigations [34]

Kinyoun was promoted, and his laboratory moved to Washington in 1892 to become the Hygienic Laboratory of the United States Public Health Service.[35]

Kinyoun's move left New York without diagnostic facilities for the port and provided Hermann Biggs with an opportunity to ask the city to establish a bacteriological laboratory. Biggs's arguments were persuasive. A cholera epidemic was threatening to spread from Hamburg, following a typical route for the disease, and effective quarantine procedures required appropriate diagnostic facilities.[36] Furthermore, he explained that the laboratory would in no way interfere with the city's private practitioners, and made elaborate provisions to guard against such an occurrence.[37] Biggs received permission in September 1892 to establish the laboratory and was granted three thousand dollars annually for the laboratory's operation.[38] He became head of the Division of Pathology, Bacteriology, and Disinfection, instituting routine tests for cholera and taking over the well-established procedures for disinfection. Testing was expanded early the next year, when the division was instructed to identify typhus cases, as well.[39] Biggs's aim was to apply bacteriological techniques as widely as possible, and he rid himself of his disinfecting duties in 1894.

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[31] New York Board of Health, *Annual Reports,* 1890–1930; David A. Blancher, "Workshops of the Bacteriological Revolution: A History of the Laboratories of the New York City Department of Health, 1892–1912" (Ph D diss , City College of New York, 1979)

[32] Joseph J Kinyoun, "Report," *Annual Report of the U S Marine Hospital and Public Health Service,* 1895 See also Oliver, *Man Who Lived for Tomorrow,* pp 105–6, Paul F Clark, *Pioneer Microbiologists of America* (Madison· University of Wisconsin Press, 1961), pp 204–33

[33] A. M. Stimson, "A brief history of bacteriological investigations of the United States Public Health Service," *Pub Health Reports,* supplement 141 (Washington, D C Government Printing Office, 1938)

[34] Kinyoun, "Report."

[35] See Ramunas Kondratas, "The U S Hygienic Laboratory," (Paper presented at the Smithsonian Institution, March 1980)

That same year Biggs visited Europe on one of his occasional trips to keep in touch with laboratory work in Paris, Berlin, Munich, and elsewhere. In France he learned of Emile Roux's technique for producing large quantities of diphtheria antitoxin in horses, and he recognized this as a considerable improvement over Emil Adolf von Behring's methods.[40]

Kinyoun also visited Roux and Behring during the summer of 1894. Roux gave him a tour of the laboratories at the Pasteur Institute and discussed the Budapest serum therapy paper Kinyoun was convinced by Roux, and sent reports of Roux's techniques to Washington. While he was in Paris, Kinyoun obtained permission from the Public Health Service to buy apparatus that would be necessary for the production of diphtheria cultures.[41]

Kinyoun's perception of diphtheria antitoxin as something more than the application of bacteriology to medicine was typical of the pioneers of diphtheria antitoxin production in the United States. He immediately saw the antitoxin's commercial significance and conceived of massive inoculation as a necessary part of public health work. That the antitoxin could not be mass produced at the time, and that there was no sense of appropriate dosage or production standards, did not dissuade him from this point of view.[42] In November 1894 he traveled to Berlin to meet Robert Koch, Paul Ehrlich, and August Wasserman, witnessed some of Behring's experiments, and sent back information on Behring's method of serum production. When he returned, he added German equipment to his laboratory and began giving instruction on how to produce antitoxin. The Washington laboratory had its first usable antitoxin later that winter, but the laboratory was already behind New York in production[43]

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[36] Charles E Rosenberg, *The Cholera Years The United States in 1832, 1849 and 1866* (Chicago University of Chicago Press, 1962), p 57

[37] Blancher, "Workshops"

[38] New York Board of Health, *Annual Reports,* 1892, Charles-E A. Winslow, *The Life of Hermann M Biggs, M.D, D Sc, LL.D Physician and Statesman of the Public Health* (Philadelphia Lea and Febiger, 1929), pp 95–98, and Oliver, *Man Who Lived for Tomorrow,* p 103

[39] Oliver, *Man Who Lived for Tomorrow,* p 137

[40] H Zeiss and R. Bieling, *Behring Gestalt und Werk* (Berlin Bruno Schultz, 1941), p 181

[41] Kinyoun, "Report."

[42] *Ibid* There were significant problems with large-scale production For example, the pH level was not specified, and the methodology for achieving proper aeration and mixing was not defined.

William H Park made the first diphtheria antitoxin outside Europe in 1894 in the New York City Department of Health laboratory.[44] Park had been appointed chief bacteriologist in 1893 and had concentrated on diphtheria diagnosis. Biggs sent instructions directly to him from Europe on methods of preparing the antitoxin, which Park implemented immediately. By the time Biggs returned, Park had already produced the serum. So successful was this new venture that in 1895 the laboratory was divided into a facility for the production of antitoxin and a laboratory for diphtheria and tuberculosis diagnosis.[45] Early in March 1895, Biggs persuaded the state senate to amend the existing laws that would have prevented the commercial production of diphtheria antitoxin. Income from this business was subsequently used to supply the laboratory with equipment and to allow the free distribution of antitoxin throughout New York City[46]

In Philadelphia (and also in Boston[47]), there was much interest in the news that effective antitoxin was being produced in the United States.[48] The scientifically oriented public health activists of Philadelphia hoped that they would now be allowed to establish an advanced scientific laboratory and overcome longstanding opposition to bacteriology. As the Board of Health's Sanitary Committee put it at the 28 October 1894 meeting, the discoveries of Behring, Ehrlich, and Roux "had the effect of stimulating efforts in the directing of the establishment of laboratories, primarily for furnishing diagnostic tests in diphtheria for the cultivation of the toxin of diphtheria and also to the cultivating and supplying [of] the antitoxin."[49]

The board itself was showing greater interest in bacteriological work: at its previous meeting on 16 October 1894, for example, a resolution had been passed which stated that a physical examination alone was not adequate for the determination of tuberculosis in cattle. A resolution was passed requiring the chief inspector of milk, one Dr. Woodward, to deem acceptable only herds that had been tested for tuberculosis by a trained veterinarian using bacteriological techniques, this in recognition of the efficacy of the tuberculin test.[50] It had, then, become essential to test for tuberculosis bacteriologically, and it was necessary to adopt a similar approach to diphtheria. Public health employees could again actively participate on a citywide scale to aid physicians in their work. The establishment in New York of separate diagnostic services provided the necessary impetus for the founding of the bacteriological laboratory so long sought by the Philadelphia scientists. The Board of Health in Philadelphia noted enthusiastically that in New York a major obstacle had been surmounted when bacteriology gained the approval of the medical profession.[51]

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[43] Hygienic Laboratory Records, National Archives, Washington, D C The Smithsonian has one of the early bottles of antitoxin produced at the laboratory, dated 1895

[44] Oliver, *Man Who Lived for Tomorrow*

[45] Blancher, "Workshops"

[46] *Ibid* In 1894, Henry Walcott organized a laboratory to produce diphtheria antitoxin for use in the state of Massachusetts, Dr J L Goodale was appointed to set up the laboratory, and Theobald Smith was assigned to the position of director for the following year See Rosenkrantz, *Public Health,* pp 113–15

[47] Rosenkrantz, *Public Health*

[48] Board Minutes, 28 October 1894

[49] *Ibid*

[50] *Ibid*, 16 October 1894

A motion was raised in 1894 for the establishment of a diagnostic laboratory for the city of Philadelphia in which bacteriological tests could be undertaken on tuberculosis and diphtheria. William Ford approved the motion and delegated the Sanitary Committee to supervise the laboratory's establishment.[52] At first the committee approached the University of Pennsylvania, whose faculty included bacteriologists, and began negotiations for the use of facilities in the new Laboratory of Hygiene. The city's work was to be separate from the university's academic tasks, and the Board of Health would provide all of the staff and facilities needed to run the laboratory.[53] The move into the university facility was postponed until the beginning of the following year; in the meantime, provisions were made for a temporary but exclusively city-operated laboratory on the seventh floor of City Hall.[54]

In November 1894, before the site of the laboratory had been settled, the Sanitary Committee reported that as a way to save time, it had ordered ninety dollars' worth of Behring's diphtheria antitoxin directly from Berlin for use at the Municipal Hospital. The committee asked for and attained approval of this action. This first batch of antitoxin was used to treat sixteen people suffering from advanced diphtheria, five of whom died.[55] The discouraging results of treatment were presumed to be a consequence of the fact that the injections had been administered so late in the course of the disease.[56] However, the mere fact that patients with advanced cases were being treated is indicative of the nature of the treatment of diphtheria in hospitals: cases were not usually seen until they were well advanced, much more advanced than Behring and Roux believed could be treated effectively by the antitoxin.[57] In addition to making the antitoxin, the Board of Health therefore needed to ensure that diphtheria cases were identified and treated quickly.

Other problems were encountered in the production of the medicine. Objections to the establishment of bacteriological facilities, especially for the production of antitoxin, were still being voiced. One physician, Elmer

[51] *Annual Report,* 1894, pp 127–28

[52] Board Minutes, 7 November 1894

[53] *Ibid*

[54] *Ibid* See also McFarland, "Beginning of bacteriology," p 182

[55] *Annual Report,* 1894, p 139

[56] Most of these injections were given after the fourth day of the disease Much literature at the time showed that this was too late, and that better results were achieved when the injections were given earlier See, for example, E L Larkins, "The use of antitoxin in the treatment of diphtheria and membranous croup with a collective report of one hundred and thirty-two cases," *JAMA,* 1896, *27* 7–10

[57] The Municipal Hospital generally only treated advanced cases

Lee, claimed that the procedure obscured the significance of the disease and was leading the profession astray.

> There are two varieties of diphtheria which exist to-day, but, one of which did not exist yesterday. One is true clinical diphtheria, the gross symptoms of which are entirely sufficient for practical diagnosis. The other form is the diphtheria of the bacteriologic laboratory. One is diagnosticated by the clinician, while the other is discovered by the bacteriologist. These two varieties are made necessary by reason of the claims of bacteriology, by which discoveries practical medicine has been both aided and retarded. Aided, by helping to a clearer understanding of the pathogenic and bacteriologic changes during the process of disease, and hindered, for the time being, by throwing a cloud across the pathway of natural and practical therapeutics[58]

Lee held that bacteria were byproducts of disease, and that therefore their identification could aid in diagnosis but not in treatment. Since bacteria were not, in his view, the cause of disease, their neutralization through the use of antitoxin served only to mislead, not to cure. Lee also criticized bacteriology in principle: "If you undertake to say to me or to the world, that that treatment is based upon scientific knowledge, it is my contention that it is an incorrect statement. If you base it upon empiricism, there is nothing more to be said. It is contrary to physiologic law."[59]

The reaction against bacteriology was only one form of objection to the city's public health initiative. Some Philadelphia physicians expected at least the level of non-intervention from the city's Board of Health which the New York City Health Department promised. Many physicians remained concerned that public health activities would interfere with private clinical medicine.

PRODUCTION OF ANTITOXIN IN THE PUBLIC SECTOR

Despite this opposition, the Philadelphia Board of Health had taken on a new bacteriological function, and therefore it decided to appoint someone to work exclusively on bacteriological problems. In 1894 it chose Joseph McFarland (1868–1945), who had earned a medical degree at the University of Pennsylvania, where he had studied pathology with William Osler. McFarland had traveled to Germany to work in medical laboratories in Heidelberg and had studied bacteriology at Halle.[60] Upon his return to the university, McFarland was appointed an assistant in pathological histology and soon became a demonstrator of pathological histology and a lecturer on bacteriology.[61]

With his European background, McFarland had ambitious bacteriological plans: he intended to produce sufficient antitoxin for the entire city. He soon became dissatisfied with the city's limited resources in the laboratory shared with the University of Pennsylvania and therefore exploited his position as a public official and looked for large, city-owned stables.

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[58] Elmer Lee, "The fallacy of antitoxin treatment as a cure for diphtheria," *JAMA*, 1896, *27* 17–18

[59] *Ibid* [discussion section], p 25

[60] Stanley P Reimann, "Memoir of Joseph McFarland," *Trans Studies Coll. Phys. Philadelphia*, 4th ser, 1946–47, *14* 133–35

[61] Curriculum vitae, Joseph McFarland Papers, College of Physicians of Philadelphia.

Initially McFarland used stables provided by the fire department, but when these proved inadequate, he was given permission to use those of the police department, a move facilitated by the close ties between the Board of Health and the police.[62] Still dissatisfied with the facilities, and unable to wrest sufficient funds from the city, McFarland became disenchanted with the board's work. In 1895 he moved to the H. K. Mulford Company to begin the commercial production of biologicals, taking with him the expertise he had gained with the city.[63]

McFarland's appointment to the Board of Health marked a milestone in the acceptance of bacteriology. As McFarland wrote in 1895, it was becoming the custom to send samples of secretions, tumors, and other pathological materials to the nearest laboratory for bacteriological examination. Inundated with work in his own department and convinced of the importance of bacteriological examination, he published the following statement to urge the local medical societies to establish a research laboratory for their own use.

> In order to overcome the expense item, I would suggest that the medical society make the pecuniary outlay, purchase the apparatus, appoint a committee to be custodians of it, and invite the *best adapted man* to use it. Many city organizations have adopted this plan in part. At present I can remember that the Philadelphia Pathological Society owns a microscope, and has a committee on morbid germs, whose office it is to report upon whatever specimens are referred to them, and that the Obstetrical Society has a paid microscopist with a similar function.[64]

McFarland viewed the new bacteriologist as an important professional who was to be treated accordingly: "When I say the pathologist or microscopist should receive a just compensation, I do not mean [merely] a living salary."[65]

The minutes of the board for 16 February 1895 record the appointment of Benjamin Meade Bolton of Baltimore to replace McFarland as chief of the newly created Division of Pathology, Bacteriology, and Disinfection at an annual salary of four thousand dollars.[66] A number of articles appeared in medical journals that year announcing the establishment of the laboratory and describing its work.[67] For example, E. O. Shakespeare, professor of medicine at the university, expressed approval of the city's laboratory in the *Dietetic and Hygienic Gazette* of 1895. He mentioned the appointment of Bolton and gave credit to the new facility for finding someone with particularly prestigious credentials. The article served as an advertisement for the laboratory and provided an organizational blueprint for other such institutions.[68]

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[62] The board had inadequate resources from the beginning and used the stables of both the fire department and the police force between 1894 and 1896 See Board Minutes, 1895, and McFarland, "Beginning of bacteriology," pp 182–83

[63] Jonathan M Liebenau, "Scientific ambitions the pharmaceutical industry, 1900–1920," *Pharmacy in Hist*, 1985, *27*: 3–11

[64] Joseph McFarland, "Bacteriologic examinations in medicine and surgery," *Trans Med Soc State Pennsylvania*, 1895, *26* 319–21 The quote is on p 321

[65] *Ibid*, p 321

[66] Board Minutes, 16 February 1895

Bolton's first task in his new position was to shore up the reporting procedures for contagious diseases which were so important for the operation of the division.[69] An act of 1818 had required that contagious diseases be recorded by the state, but this requirement was enforced only sporadically, and records were fragmentary.[70] A new act of 1895 provided for prompt reporting, isolation facilities, regulation of the attendance of sick schoolchildren, guides for disinfection, interdiction of burial for those who had died from infectious diseases, vaccination guidelines and recommendations, and penalties for failure to comply with these directives.[71]

Under Bolton, a new city laboratory was completed in City Hall and began its work by soliciting samples from physicians.[72] The city laboratory was becoming a center offering a variety of services for private physicians, a function later assumed by some drug companies.[73] In addition to equipment used in the diagnosis of diphtheria, there were facilities for the examination of blood, urine, gastric secretions, sputum for tubercle bacilli, and other pathological material. Plans for full-scale bacteriological tests of the city's water supply were also made.[74]

The production of diphtheria antitoxin continued to be a high priority The Philadelphia Board of Health hoped to produce enough antitoxin to supply the needs of the city, as New York was able to do. In May 1895 the laboratory began to prepare diphtheria antitoxin for distribution in the manner pioneered by the New York City program. Toward the end of the month the city's chemistry laboratory moved into rooms in City Hall nextdoor to the bacteriology laboratory, and a combined facility was inaugurated.[75] The board also recognized the significance of Park and Biggs at

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[67] B Meade Bolton, "Theory of serum therapy contribution from the bacteriologic laboratory of the City of Philadelphia," *JAMA*, 1896, *27* 26–27 See also note 105, below

[68] Edwin O Shakespeare, "The new municipal laboratory of bacteriology in Philadelphia," *Dietetic and Hygiene Gaz.*, 1895, *11* 223–24

[69] Bolton, "Theory of serum therapy"

[70] *Annual Report*, 1895, pp 136ff

[71] *Ibid*, pp 224–25

[72] Board Minutes, 21 May 1895 Notices of the laboratory's services were sent to physicians throughout Philadelphia, copies are in the city's archives

[73] Liebenau, "Scientific ambitions"

[74] *Annual Reports*, 1895, 1896 See also "Report of the Antitoxin Laboratory," *Annual Report*, 1907, pp 17–18

[75] Board Minutes, 12 March, 21 May, 11 July 1895

New York as personal leaders and hoped that Bolton would be as good a director.[76]

The manner in which the laboratory was used was presented well in an article by Dr. William Duffield Robinson titled "Some Experiences with the Bacteriological Department of the Philadelphia Board of Health."[77] One of his patients had ambiguous signs of diphtheria. The patient's throat culture was examined in the laboratory and showed the Klebs-Loeffler bacillus. The patient was treated while cultures were examined from the other members of the family. All of them had diphtheria, and its presence was diagnosed quickly enough to prevent its spread to neighbors and to allow immediate therapy. A second case involved a patient who had classic symptoms of diphtheria but whose tests proved negative. The patient soon recovered without medication. Robinson made use of other tests available by 1896. One of his patients had a history of pneumonia and was again displaying symptoms of the disease. Sputum tests showed that there were no pneumococci present, but that the patient was infected with tuberculosis. He was soon "practically moribund," having contracted what was diagnosed as fulminant miliary tuberculosis.[78]

The city's disinfector was made responsible to the chief bacteriologist in 1895, since the board, on Bolton's advice, decided that it was necessary to conduct full bacteriological tests on objects disinfected, and the duty of prescribing the most appropriate disinfectant was assumed by the bacteriology laboratory.[79] This move paralleled a similar shuffling of bureaus in New York.[80] As the capabilities of the laboratory expanded, its duties were more precisely defined. They were:

1. The examination of pathological material sent by physicians.
2. The preparation of diphtheria antitoxin.
3. Experimental work bearing upon questions of interest in bacteriology.[81]

Physicians were notified of the availability of culture tubes in police stations and were given cards to return with specimens to the laboratory.[82] Detailed directions for taking sputum tests and giving inoculations were provided with the materials. The kits also contained a sterile swab and a sealable glass container. Physicians were informed that they could receive help from the staff of the laboratory if they did not wish to administer the tests or give the inoculations themselves.[83]

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[76] *Ibid*, 12 March 1895

[77] William Duffield Robinson, "Some experiences with the bacteriological department of the Philadelphia Board of Health," *Med. News* [New York], 1896, *68* 551–52

[78] *Ibid.*, p 552.

[79] *Annual Report*, 1895, p. 141

[80] Oliver, *Man Who Lived for Tomorrow*, pp 138ff

[81] *Annual Report*, 1895, pp. 142–43

[82] *Ibid*

[83] *Ibid*

Police stations were used in this campaign because they were the public offices most evenly distributed around the city. The police had assisted the Board of Health for some time, enforcing public health laws and serving as auxiliary sanitarians. Samples would be either left with the police for transfer or sent directly to the laboratory. Diagnostic results would usually be available the next day. If the physician certified that the patient was needy, he could obtain antitoxin free from the department. The antitoxin also came in a small kit with instructions. The physician was expected to keep case records, as the board requested information concerning treatment, including the stage the disease had reached; the amount of antitoxin used; any immediate reaction to it; and the final outcome of the measures taken.[84]

By the end of its first year, the Public Health Laboratory was functioning as a diagnostic and information center for Philadelphia physicians. It was also producing diphtheria antitoxin, although in smaller quantities than at the New York laboratory. As these duties became further developed, the laboratory began to encroach even more into commerce.

COMMERCIALISM IN THE BOARD OF HEALTH

The Board of Health, or at least its laboratory, was acting as a pharmaceutical manufacturer, and its commercial activities soon caused conflicts of interest. One of the duties of the new laboratory was to develop and test chemicals for public health use.[85] In 1896, Dr. P. D. Keyser, a member of the Sanitary Committee, ordered Bolton to test the proprietary disinfectant Electrozone.[86] Keyser pressured Bolton to certify the product, which would then be sold to the board for use in disinfecting homes and public schools. Bolton resigned his post in protest against this clear effort to abuse the laboratory's official position in order to endorse what was simply a hypochlorite solution made by electrolyzing sea water.[87] However Bolton's assistant, W. J. Gillespie, conducted the tests and gave Electrozone an exceedingly favorable review.

> These experiments proved Electrozone to be a powerful germicide since it kills the most resistant of all organisms, anthrax spores, in 5 minutes and less.
>
> Results in practical work, no doubt, will prove Electrozone to be more effective than corrosive sublimate for all organisms, owing to the fact that corrosive sublimate will form an inert insoluble precipitate with all albuminous matter,

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[84] *Ibid*, Board Minutes, 11 July 1896

[85] This work, formerly conducted in the board's chemical laboratory, was done in the bacteriological laboratory after 1896

[86] Board Minutes, 11 July 1896

[87] *Ibid*

> thus diminishing its power and energy as a germicide. Electrozone forms no such insoluble precipitate.[88]

Bolton returned as chief of the division after the Electrozone episode, only to complain about the episode so bitterly at board meetings that he was soon removed from his position.[89] Gillespie left the laboratory to work in the production of the sea water disinfectant at the Electrozone Commercial Company.[90] This was just the sort of incident some members of the board had apprehended previously, and it confirmed Bolton's view that commercial activities were inappropriate.

The laboratory was permanently weakened by this episode. Alexander Abbott of the University of Pennsylvania's Laboratory of Hygiene was appointed to replace Bolton in January 1897. However, the job was reclassified as a half-time position paying only two thousand dollars per year.[91] Support for the bacteriological work had markedly declined, as the city government became increasingly resistant to the continuing agitation for expanded facilities for production of diphtheria antitoxin. The Electrozone affair provided the impetus for the reduction of appropriations, and Abbott's assistant, H. D. Pease, was forced to move to the University of Pennsylvania's laboratory part time.[92] Abbott was expected to initiate fewer large, expensive projects. The board had to scale down aspects of the division's work, but it managed to avoid downgrading the laboratory's status. However, Abbott's efforts were impaired because the initial momentum of the laboratory had been lost, and he was eventually unable to maintain antitoxin production at the level demanded by the public health service and the doctors who had come to rely on the board for their supply.[93]

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[88] *Ibid.* See also *Annual Report*, 1895, p. 39 "During the latter part of the year [1894], an apparatus for the manufacture of hyperchlorite of sodium in solution has been constructed in the basement of the City Hall, where the facilities for obtaining the required amount of electricity are at hand. This material is manufactured by electrolytic action upon a solution of chloride of sodium, or common salt. The tank contains 500 gallons of liquid, which is the amount used daily in the disinfecting tank. This material is well known for its valuable properties, and the process of manufacture is novel and economical."

[89] Board Minutes, 11 July 1896

[90] *Ibid.* Gillespie came back to the board representing the Electrozone Commerical Company three years later (Board Minutes, 1 November 1898)

[91] *Annual Report*, 1897, p. 168

[92] Pease moved to the University of Pennsylvania in 1896 (University of Pennsylvania Catalogue)

[93] *Annual Reports*, see especially 1906, p. 18. The production of antitoxin increased beginning in 1896, but amounts still fell far short of the needs of the public health program. Production was as follows

Year	Units Produced
1896	900,000
1897	2,334,000
1898	3,367,000
1899	2,976,000
1900	7,849,500
1901	8,061,000
1902	8,656,000
1903	11,013,000
1904	15,447,000
1905	21,800,000
1906	34,000,000

Beginning in 1897, the Public Health Laboratory became more involved in the variety of tasks which now came to be associated with bacteriological testing, and less attention was paid to the production of diphtheria antitoxin. Since Abbott was attached to the Laboratory of Hygiene as well as to the city Board of Health, he was able to continue extensive projects. He divided his work between the two institutions.[94] He also repeatedly petitioned the board and the city for funds to re-expand the diphtheria antitoxin production facilities.[95] He had no success in this endeavor, although he was able to hire an assistant to work on such newly expanded tasks as the administration of the Widal tests for the diagnosis of typhoid fever. His appointee, Alonzo Stewart, was Adjunct Professor of Bacteriology in the Philadelphia Polyclinic and College for Graduates in Medicine. Stewart divided his teaching and research in a manner similar to Abbott's.[96] The board's *Annual Report* for 1897 noted that the laboratory's activities had been extended by the routine administration of Widal tests and bacteriological analyses of water for the Bureau of Water. More samples of milk were also tested for the inspector of milk, and, the report noted, the diagnosis of tuberculosis continued to be a problem.[97] The laboratory analyzed 563 specimens of sputum but did not classify the results because of the belief that no action would be taken in any case.[98]

Abbott then appointed an assistant for the routine examination of water and milk, to administer the occasional tests being undertaken for other offices, and to take in occasional private business.[99] Between March and December 1897, 110 samples of water were tested, 16 for private firms. They showed wide variation depending on season and rainfall, ranging between 115 and 14,440 bacteria per cubic centimeter.[100] Tests showed similar variations in milk. Bacterial counts in 221 samples ranged from 1,200 to 819,000 per cubic centimeter, with an average of 121,497.[101]

The largest area of expansion was in diagnostic testing for diphtheria In

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After 1907, the department bought large quantities of antitoxin from the New York City Health Department and from Lederle Laboratories

Year	Units Produced	Units Purchased
1907	33,160,000	3,658,000
1908	8,269,300	32,400,000
1909	5,257,050	67,032,150
1910	14,678,600	79,984,800

Annual Reports, 1909, p 423, 1910, pp 462–63

[94] *Annual Report,* 1906, p 18
[95] *Ibid,* Thomas D Davis, "State Board of Health and antitoxin," *Pennsylvania Med J,* 1899, *12* 152
[96] *Annual Report,* 1906, p 18
[97] Board Minutes, 19 January 1897
[98] *Annual Report,* 1897, p 166
[99] *Ibid,* p 168
[100] *Ibid*
[101] *Ibid.*

1897, 7,313 cultures were taken from 3,353 persons suspected of having the disease, of whom 2,020 proved to be infected.[102] In 1896 a special Diphtheria Pavilion had been established at the Philadelphia General Hospital, in which antitoxin was administered to promising cases. In the pavilion's first year, 869 patients were admitted, of whom 184 died at the hospital, a death rate of 21 percent.[103] Abbott hoped that the new facility, together with the increase in testing, would encourage City Hall to recognize the necessity of expanding the production of diphtheria antitoxin. The mayor's office was willing for the department to produce its own antitoxin but still refused to fund the operation sufficiently. The city government was particularly concerned that the board was competing with private enterprise and was unwilling to provide the capital investment needed. However, the strategy of restricting funds proved short-sighted, because, far from undercutting commercial producers through massive production, the Philadelphia Board of Health became chronically short of antitoxin and was forced to turn to William Park and the New York City laboratory for supplies. This had become an accepted procedure in public health departments around the country, and the New York laboratory responded by continually enlarging its production.[104]

REACTIONS TO DIPHTHERIA ANTITOXIN

Just as the Philadelphia Board of Health was failing to meet demand for antitoxin, interest in the therapy reached new heights. Diphtheria antitoxin was becoming available in large quantities, and even if tests proved inconclusive and there were problems with the form and concentration of the serum, physicians needed to take diphtheria antitoxin into consideration. Medical men were gradually agreeing upon certain results of treatment, and it was these that antitoxin producers, whether public health bodies or companies, had to address.

The year 1896 marked an expansion in public awareness of diphtheria and of the possibility of effecting a cure. The *Journal of the American Medical Association,* for example, published a number of articles on diphtheria antitoxin reporting on the first full year of use. These papers, read at state medical society meetings in Illinois, Indiana, and Michigan, and at the AMA convention in Atlanta, contained some of the most complete summaries of findings and the most open debate yet presented as to antitoxin's effectiveness[105] Central to the debate was the use of statistics on the mortality rate for diphtheria after the application of antitoxin. Hospital figures seemed particularly unreliable, because usually only the worst cases were admitted, and it was often difficult to ascertain the date of onset of the disease. Moreover, diphtheria tended to follow an erratic course; the general strength of victims seemed to be most critical to recovery. This factor, as well as the varying stages at which the disease was treated, seriously complicated results. Conventional treatments, which strengthened the body, attempted to keep fever down, and attempted to keep the throat clear, seemed to have some effect. When the antitoxin was being used experimentally, there was a wide range of practice, especially as to how long after the onset of the disease the medicine had been administered. The virulence of the preparations varied by a factor of 1,000, and the methods of application also differed widely.[106]

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[102] *Ibid.*

[103] Welch, "Municipal Hospital," in Henry, ed., *Founder's Week,* pp 546–47, 560

[104] John Thomas Mahoney, *The Merchants of Life An Account of the American Pharmaceutical Industry* (New York Harper, 1959), p 163, *Annual Report,* 1910, pp 462–63

[105] The papers can be found in *JAMA,* 1896, *27* E Fletcher Ingals, "Orrhotherapy in diphtheria," 1–4, Charles T McClintock, "The outlook in serum therapy," 4–7, Larkins, "Use of antitoxin," 7–10, Edwin Rosenthal, "Serum therapy in diphtheria," 11–14; Harold C Ernst, "An introduction to the discussion upon

Worldwide statistics, which were frequently assessed during the first few years of use of antitoxin, ranged from suspiciously good for Behring and his compatriots, through reasonably favorable for the Americans and the French, to downright appalling for the London hospitals.[107] The explanations given for the particularly bad results obtained by the British were that they used weak and unstandardized serum (supplied by the Wellcome Company),[108] and that they insisted on treating even advanced cases in very young children.[109]

When Americans analyzed the statistics, even those for the London hospitals, they generally accepted that a clear advantage was offered by the antitoxin for treatment of strong children over ten years old who were treated between the first and fourth day of their affliction. In the Philadelphia Municipal Hospital, William M. Welch admitted only "cases considered favorable."[110] Statistics from the New York and Chicago boards of health, like those from many other large cities, were considered unsatisfactory because many physicians did not report cases of diphtheria until they feared the patient was going to die, "whereas physicians who obtain antitoxin from the Board of Health would necessarily report nearly all cases where it is to be employed. It is therefore not at all improbable that the percentage of

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'blood-serum therapeutics,'" 14–17; Lee, "Fallacy of antitoxin treatment," 17–19, Louis Fischer, "Some practical points on the combined effects of antitoxin and intubation, with special reference to infant feeding in malignant diphtheria," 19–20, Joseph William Stickler, "An experience with antitoxin with instructive results," 20–21, Edwin Klebs, W E Casselberry, Dr Ross, Dr Thomason, J A. Larrabee, W J Bell, H. E Garrison, Dr Baker, Dr Hodgkis, D C Wilson, Dr Knipe, Elmer Lee, W A. Dixon, "Discussion on papers of Drs Elmer Lee, Joseph W. Stickler, and Louis Fischer," 21–26, Bolton, "Theory of serum therapy," 26–27, L. Emmett Holt, W P. Northrup, Joseph O'Dwyer, Samuel S Adams [committee members], "Report of the American Pediatric Society's collective investigation into the use of antitoxin in the treatment of diphtheria in private practice," 27–35 See also, in the same issue of *JAMA*, E Fletcher Ingals, "Orrhotherapy in diphtheria [letter to the editor]," 49, and P. M Bracelin, "The Bracelin remedy in diphtheria [letter to the editor]," 49–50

[106] Larkins, "Use of antitoxin."

[107] Rosenthal, "Serum therapy", Ernst, "'Blood-serum therapeutics'"

[108] "Report," *Brit Med J*, 1896, *1* 252

[109] Rosenthal, "Serum therapy"

[110] *Annual Report,* 1898, pp 90–94; Ingals, "Orrhotherapy," p 2

mortality as given for cases in which antitoxin was not used is two or three times larger than it should be."[111]

Especially when results were aggregated, they proved particularly ambiguous. Early statistical reports indicated quite optimistic results in Philadelphia, where only the most appropriate patients were given the antitoxin. Statistical reports from other cities showed a higher death rate with antitoxin than without it.[112] Although one commentator condoned the use of the antitoxin as a curative but not as a prophylactic, he concluded that antitoxin certainly had not been proven a specific for diphtheria.[113]

Some clarification of the results of using antitoxin occurred with improvements in the quality of the serum in the late 1890s. However, problems still occurred with its administration. In 1895 a good batch had 100 units of antitoxin per cubic centimeter, but the units were increased fivefold in concentration by the next year. Severe rheumatic pains and other discomfort accompanied the use of antitoxin of this concentration. Controlling the concentration of the antitoxin was therefore a major goal for producers, because injecting large quantities of anything was risky, or, as one physician put it, "to inject 10 c.c. of anything into a child is a heroic procedure."[114] The same physician, Charles T. McClintock, voiced the concerns of those who believed that the antitoxin should be administered in a sequence of small doses rather than large ones. The main reason for this was probably the pain it caused, but he had his theoretical reasons, too. The disease did not produce toxin all at once, he argued. The antitoxin should therefore be used to stimulate cells to destroy the toxin. This would best be achieved by a sequence of injections to nourish healthy cells.[115]

As can be seen from the general debates over diphtheria antitoxin, interest in the therapy grew significantly. As a result, commercial companies became increasingly interested in developing a market for biological drugs. Before they could do so, they needed to tackle the public health bodies in their function as manufacturers of the drugs. The latter not only were producing diphtheria antitoxin, but were threatening to manufacture other medical products—usually tetanus antitoxin, which was manufactured similarly to diphtheria antitoxin, and smallpox vaccine—in competition with commercial manufacturers.[116] It became increasingly important for commercial firms to reestablish their preeminence in pharmaceutical manufacturing.

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[111] Ingals, "Orrhotherapy," p 2

[112] In London, the most vociferous critic of the use of antitoxin was Dr Lennox Browne His view was contradicted by statistics published by the Metropolitan Asylum's board in 1895 Comparing 3,000 treated and 3,000 untreated cases, the board concluded that more than 250 lives were saved through treatment (McClintock, "Outlook in serum therapy ")

[113] Lee, "Fallacy of antitoxin treatment"

[114] McClintock, "Outlook in serum therapy," p 4

[115] *Ibid*

[116] *Annual Reports*, 1897–1900, Rosenkrantz, *Public Health*, pp 113–14, 124–26

To regain their former positions, the companies needed assistance from the public health boards As the debate over diphtheria antitoxin showed, there could be significant problems in its production. The municipal manufacturers had faced these problems from their first attempts to make the drug. They needed a certain amount of "know-how," laboratory technique and procedure, which was generally undocumented in the first published descriptions of antitoxin production. The first problems encountered by these producers, which had been overcome in Germany by 1890, were associated with the production of the toxin itself.[117] Conditions under which the diphtheria bacillus was cultured had to be maintained with precision, and although the method was relatively straightforward, variables such as acidity had to be closely monitored. Producers also needed to master the tools for the extraction of immunized blood from the horse and techniques for the isolation of blood serum from blood cells on a large scale.[118]

Park was able to overcome these difficulties in his well-appointed laboratory in New York. His experience with diphtheria both clinically and in the laboratory allowed him to develop the necessary techniques.[119] Shortly after he first successfully produced the serum, he was visited by public health officials from all over the country.[120] Commercial producers followed their lead: among Park's visitors were representatives from two drug companies, H. K. Mulford and Parke Davis. Joseph McFarland learned whatever he could on behalf of Mulford.[121] The companies took advantage of the fact that public health organizations were not, in the end, commercial enterprises, and therefore had a duty to advertise their knowledge for the general good.

Having learned what they could from public health bodies, and having lured personnel away from the municipal laboratories, commercial pharmaceutical manufacturers next sought to reduce public production. Two main arguments were used in this campaign: the first was that the municipal facilities were inadequate, and the second was that the government should not interfere with commercial enterprise by undercutting the commercial market. Firms had one marked advantage in Philadelphia: Board of Health laboratory facilities were poor and were staffed inadequately. The horses were not sufficiently nurtured, and the stables were decrepit. Although there were no cases of contaminated serum, there was constant concern over sterility.[122] These conditions, like less suspect conditions in Massachusetts, became the focus of criticism from commercial producers of antitoxin.[123] Mulford, for example, strongly intimated that public health boards were incapable of maintaining the highest standards and keeping up to date with procedures.[124]

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[117] Emil Behring, "Untersuchungen uber das Zustandekommen der Diphtherie-Immunitat bei Thieren," *Deutsche medicinische Wochenschrift,* 1890, *16* 1145

[118] Liebenau, "Scientific ambitions "

[119] Oliver, *Man Who Lived for Tomorrow,* W H Park and A. W Williams, "The production of diphtheria toxin," *J Exper Med*, 1896, *1* 164

[120] Oliver, *Man Who Lived for Tomorrow,* Blancher, "Workshops "

[121] McFarland, "Beginning of bacteriology "

[122] *Annual Report,* 1910, p 22

Together with the steadily voiced objection to government competition with private business, criticism of facilities helped the companies persuade the city governments to restrict their funding of public health laboratories. So effective was the campaign that even the New York laboratory was forced to end its sales of antitoxin by 1904.[125]

The first biological drug, diphtheria antitoxin, emerged in Europe out of scientific medicine. It was the public health bodies in the United States, not the commercial companies, which were ready to accept the challenge of antitoxin production. However, their new role as pharmaceutical manufacturers was dogged with problems: the public health bodies' greatest impediment was that they were *not* commercial enterprises, and thus were dependent upon elected city officials for their funds. These officials were peculiarly sensitive about infringing on the private sector, unlike company managers, who naturally sought to expand. Thus, in the public health laboratories' very success lay the seeds of their decline. Once the commercial firms had learned techniques from the public health boards, the firms pressed the boards to relinquish their new role. The transfer of production from the public to the private sector was effected by the new mobility of academic, public health, and commercial personnel. The medical scientists who had led in the public health production of diphtheria antitoxin subsequently transformed the companies.

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[123] Rosenkrantz, *Public Health,* p 123

[124] H K Mulford Company, *Price List,* 1900

[125] Mahoney, *Merchants of Life,* p 462

NOTES AND COMMENTS

TWINS AS MONSTERS: ALBERTUS MAGNUS'S THEORY OF THE GENERATION OF TWINS AND ITS PHILOSOPHICAL CONTEXT*

J. M. Thijssen

It has often been observed that medieval theoretical medicine and biology derive much of their conceptual apparatus from Aristotle's natural philosophy.[1] It may even be stated that in some cases a thorough understanding of the medieval theories is not possible without a knowledge of this philosophical background. This is especially true for the medieval theory which gives an explanation for multiple births, and which thus pertains to the field of embryology. As will be shown below, this embryological theory is part of a broader philosophical theory which tries to explain monstrosities in Nature.

In this paper my interest will be focused on Albertus Magnus (Albert the Great, c. 1200–1280). Although he was not a physician, Albert holds an important place in the history of medieval medical learning.[2] In his *De Animalibus,* he presents an explanation of the procreation of twins which provides answers to questions left unanswered by Aristotle's theory.[3] In order to appreciate the medieval contribution to the theory of the conception of twins, as well as Albert's specific contribution to that theory, it will be necessary first to explore Aristotle's embryological theory of twins and its philosophical background.

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[1] Cf Luke Demaitre and Anthony A. Travill, "Human Embryology and Development in the Works of Albertus Magnus," in *Albertus Magnus and the Sciences Commemorative Essays 1980,* ed James A. Weisheipl (Toronto Pontifical Institute of Mediaeval Studies, 1980), pp 409–10, M. Anthony Hewson, *Giles of Rome and the Medieval Theory of Conception A Study of the "De formatione corporis humani in utero"* (London Athlone Press, 1975), pp 67–149, and Joseph Needham, *A History of Embryology,* 2d ed., rev with the assistance of Arthur Hughes (Cambridge. Cambridge University Press, 1959), pp 56–60, according to whom Aristotle had an unfortunate influence on the progress of science Needham has a low opinion of the medieval developments in embryology (cf p 60).

[2] Nancy G Siraisi, "The Medical Learning of Albertus Magnus," in Weisheipl, ed., *Albertus Magnus and the Sciences,* pp 379–81 Albert is the only medieval thinker who is discussed in Needham's *History of Embryology*

[3] There are not many thirteenth-century commentaries on Aristotle's biological works The commentary of Albert is one of the few which is also available in a modern edition Albertus Magnus, *De animalibus libri XXVI,* ed. Hermann Stadler, in *Beitrage zur Geschichte der Philosophie des Mittelalters* (Munster Aschendorff, 1916), vol 15 (books 1–12) and (1920), vol 16 (books 13–26)

Aristotle mentions multiple offspring in his *Historia animalium* and *De generatione animalium.* The relevant passages in these works reveal that the concept of superfoetation plays a basic role in his theory of twins, which may best be illustrated by the legend of the conception of Hercules and Iphicles.[4] According to this legend, Alcmene, once upon a time, was seduced by Zeus, who on this occasion had disguised himself as Alcmene's husband, Amphitryon. The next day Amphitryon returned home and also paid a visit to his wife. Both "visits" resulted in conception, and shortly afterwards Alcmene gave birth to twins: Hercules, son of Zeus, and Iphicles, son of Amphitryon.[5] This legend is an example of successful superfoetation, that is, where a woman is impregnated more than once in the same cycle. Another instance of superfoetation reported by Aristotle is that of the adulterous wife who gave birth to twins, one resembling her husband, the other her lover.[6] For superfoetation to be successful, the interval between the first and the second conception should not be too long; otherwise the second embryo will be aborted.[7]

Of course, the development of more than one fetus may also be induced by one partner, or even after one mating. The description of polyembryony resulting from the last circumstance conforms most to modern ideas and forms the central part of Aristotle's theory.[8]

According to Aristotle, all natural generation involves form (*eidos*) and matter.[9] The specific form which the offspring is to have is contributed by the male, and in this way the male acts as the formal cause in the process of generation. The female provides the raw material which is capable of being the vehicle of this specific form, and in this way she acts as the material cause. The male and female contributions are the semen and the menstrual blood or *katamenia* (*menstrua*). The semen, being a "residue" of the blood, contains the form which shapes the female matter. The semen does not itself become part of the offspring, but evaporates. It only acts as a medium to transfer the form into the matter. In this context, Aristotle compares the generation of offspring with the production of artifacts by a carpenter. The male's role in generation is like that of a carpenter who uses tools to shape the raw material into a product.[10] At other places the formation of an embryo is compared with the process of making cheese: the spermatic secretion acts upon the female menstrual blood like the rennet acts upon the milk.[11]

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[4] For the survey of Aristotle's theory of twins, I used the following editions *Historia animalium,* trans D'Arcy Wentworth Thompson (London Oxford University Press, 1956), *De generatione animalium,* trans Arthur Platt (London Oxford University Press, 1958), *De partibus animalium I* and *De generatione animalium I,* trans and annotated by David M Balme (Oxford At the Clarendon Press, 1972), and *De generatione animalium Translatio Guillelmi de Moerbeka,* ed H J Drossaart Lulofs in *Aristoteles Latinus* (Bruges and Paris Desclée de Brouwer, 1968).

[5] In *Hist animal* 7 584a 13–15, Aristotle refers to this legend.

[6] *Ibid* 585a.12–17

[7] After conception, during the period of gestation, the uterus is supposed to close Cf *De gen animal* 773b.15 and following In *Hist animal,* Aristotle reports that the first, and not the second, embryo will be destroyed He adds that if a pregnant woman cohabits after the eighth month, the child born will in most cases be covered with a slimy fluid (cf 585a.23–25).

[8] In fact it is only one of the possible variations of superfoetation The discussion of twins in *Hist animal* 7.4 is limited to superfoetation due to different copulations In *De gen animal* 773b 6 and following, superfoetation by one single act of sexual intercourse is discussed.

[9] Aristotle, *Metaphysica,* trans William D Ross, 2d ed (London Oxford University Press, 1928) 1034a 21–b 1; 1044a 32–1044b 1

An embryo is conceived when the proper proportions of the male and the female contributions are present. A shortage or excess of one or both liquids prevents conception. Animals produce many young after one coitus when there is an abundance of male sperm and female matter. At this point in the theory, the comparison between the seed and the rennet is no longer valid, as Aristotle himself points out.[12] More rennet does not make two cheeses out of one. There also is a qualitative component in the semen, that is to say, the setting agent, which is indivisibly and completely present in each parcel of semen after the physical semen has been divided.[13] So, two factors play a role in the process of generating twins: abundance and division.

The Aristotelian explanation of the generation of twins is completely different from the one presented in *De natura pueri* of the Hippocratic corpus. There, the generation of manifold offspring is related to the manifold structure of the womb: during one single coitus the different "cells" (*receptacula/cellulae*) of the uterus are filled with male semen, which is emitted during successive spasms.[14] The text remains silent on the number of cells in the womb, but there exists a tradition in which this number is seven: three cells for the generation of girls, three for boys, and one, in the middle of the womb, for hermaphrodites.[15]

This, then, is the medical part of Aristotle's theory Let us move on to the philosophical component, which proved so influential in medieval medicine and biology.

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[10] *De gen animal* 1 730b 1–25

[11] *De gen animal* 1 729a.10–15 and 4 771b 15 and following Aristotle refuted the theory that not only the male, but also the female, contributes seed (cf *De gen animal* 729a.22–33 and the lucid commentary of Balme, in the Oxford University Press edition [1972], p 151) A short discussion of Aristotle's theory and the two-seed theory is given in Needham, *History of Embryology*, pp 42–43 The theory that the female also emits seed may be found in Hippocrates' *On Generation*, chap 5 Cf Iain Lonie, *The Hippocratic Treatises "On Generation," "On the Nature of the Child," "Diseases IV" A Commentary* (Berlin Walter de Gruyter, 1981), p 3, and his commentary on p 123 A summary of Aristotle's embryological theory is provided in Anthony Preus, *Science and Philosophy in Aristotle's Biological Works* (Hildesheim Georg Olms, 1975), pp 48–64 Johannes Morsink, *Aristotle on the Generation of Animals A Philosophical Study* (Washington, D C University Press of America, 1982) interprets *De gen animal* as being in dialectical opposition to the two Hippocratic treatises *On Semen* and *On the Nature of the Child*

[12] *De gen. animal* 772a.1–37 On 774b 1–3 Aristotle observes that hairy men more than smooth men are given to sexual intercourse and have much semen.

[13] See Wolfgang Kullmann, *Die Teleologie in der aristotelischen Biologie* (Heidelberg Carl Winter Universitatsverlag, 1979), pp 56–57, for a good summary of the problem and its context. Aristotle developed his theory in *De gen. animal* 728b 32–729a.20 Cf also the commentary of Balme, p 151, on this passage Aristotle developed this theory within a controversy over the pangenesis theory

[14] Lonie, *Hippocratic Treatises*, pp 21 and 253–54

[15] See Fridolf Kudlien, "The seven cells of the uterus the doctrine and its roots," *Bull Hist Med*, 1965, *49* 415–23, for the sources of the tradition of the seven-celled uterus

Surprising from a modern point of view is that, in *De generatione animalium,* Aristotle presents his theory of the generation of twins against the background of an exposition of the generation of *monstra* in general. The implication made here is clear: twins are some kind of monsters.[16] By monsters, Aristotle does not just mean creatures which, due to some pathological process, are misshapen, but, much more generally, all creatures which are out of the ordinary in the sense that they are not the result of the common course of Nature. To use medieval terminology, monsters are creatures beyond the range of Nature (*praeter naturam*). They are the faults of nature (*peccata naturae*).

The notion of *monstrum* has a place within the context of Aristotle's teleology: Nature always acts for the sake of ends (*causa finalis*). This purposiveness of Nature is manifest when animals like spiders build webs and swallows build nests as if their doing so were the work of reason. In reality, these actions are the manifestations of the final cause which is at work in Nature. Sometimes, however, the purposiveness of Nature may be faulty; then, due to certain causes, the intended end (*finis*) may not be achieved, and in that case the outcome of a natural process is called a monster.[17]

Aristotle's teleology depends upon the observed regularity of Nature. If something irregular happens, that is, something outside the common course of Nature, we know that the purpose was not achieved. The common course of Nature is identified with those occurrences which, according to observation, are most frequent. So, a monster is not only the outcome of a corrupted natural process, but also an outcome which does not occur very often. From the thirteenth century onward, after Aristotle's works became known in the West, this view of the natural order of the world was the accepted view. The medieval expressions *praeter naturam* (outside Nature's common course) and *praeter ut in pluribus* (outside that which occurs frequently) are mutually interchangeable.[18]

Against this background it is clear why twins are considered to be monsters. The birth of twins is something exceptional. It is a phenomenon much like the birth of an infant who has eleven fingers. Nearly always (*ut in pluribus*) a human being has ten fingers, but sometimes (*ut in paucioribus*) a human being may have eleven fingers. In the latter case, however, something has gone wrong in the common course of the causal nexus.

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[16] *De gen. animal.* 771a.15 and following

[17] Cf Aristotle, *Physica,* trans Robert P Hardie and R. K. Gaye (London. Oxford University Press, 1930), 2.8, and especially 199a.33–199b 27

[18] *De gen. animal* 770b 9–11 "Est autem monstrum aliquid eorum que preter naturam, preter naturam autem non omnem, sed eam que ut in pluribus" Cf also 772a.35–772b 1 For a discussion of Aristotle's teleology I refer the reader to Balme, pp 93–98, Kullmann, *Die Teleologie,* Preus, *Science and Philosophy,* pp 183–221; and Richard Sorabji, *Necessity, Cause, and Blame Perspectives on Aristotle's Theory* (London Duckworth, Ithaca, New York Cornell University Press, 1980), pp 143–85 See Anneliese Maier, *Die Vorlaufer Galileis im 14 Jahrhundert Studien zur Naturphilosophie der Spatscholastik* (Rome Edizioni di storia e letteretura, 1949), pp. 223–29, for the related medieval theory of necessity and contingency in works not biological.

Aristotle even argues that both the unnatural multiplication of body parts (like fingers) and the birth of twins are caused by an abundance of matter.[19] The only difference is that in the case of twins, there is also a *division* of that matter. He remains silent on the causes of that division.[20]

It is at this point that Albert the Great makes his contribution to the theory of the generation of twins. In *De animalibus* 18.2.2, which is a commentary on Aristotle's theory of twins as explained in *De generatione animalium,* Albert in fact summarizes his own theory of twins.[21] Like Aristotle, he is of the opinion that the generation of twins is caused by abundance (*superfluitas*) and division (*divisio*) of matter. Unlike Aristotle, however, he briefly enumerates the causes of the division of the matter before referring his readers to another part of his work, where he has treated this subject more completely (*sicut in antehabitis huius scientiae determinatum est a nobis*). Without doubt, Albert is referring here to *De animalibus* 9.2.6. This chapter, a digression within Albert's commentary on Aristotle's *Historia animalium,* is devoted to a causal explanation of the division of the matter.

My exposition of Albert's theory of twins will concentrate on *De animalibus* 9.2.6 for two reasons: (1) the theory expounded in this digression is a genuine contribution to Aristotle's theory, and (2) this text provides an opportunity to clarify its philosophical background, especially in connection with Albert's notion of monsters.[22] First I will discuss the embryological part of Albert's theory.

In his theory of twins, Albert attributes an important role to the division of matter, or, perhaps better, the division of the generative material (*materia generationis*). The *materia generationis* is the mixture of the male and female contributions to the product of conception. This mixture is also called *sperma* by Albert. Note, however, that in the text I am using, the term *sperma* is used interchangeably for the mixture (zygote) as well as for either the male or the female contribution. In *De animalibus* 9.2.6, Albert enumerates the causes for this division. He distinguishes between causes which originate with the male and causes which originate with the female.

The first cause of the division of the generative material is the fact that the male sperm is not emitted in a continuous flow, but in successive impulses (*impulsiones*) which diminish in force; in this way, sperm reaches different parts of the womb. The "impulses" of male sperm are attributed to "interruptions of the spirit," which are due to the fact that the "spirit" has to come from different parts of the body. From other texts it is clear that Albert conceives *spiritus* as an instrument which conveys the biologically essential virtues from the principal parts of the body (heart, liver, and brain) to the rest of the body. The "spirit" is separated from the soul only in a functional sense, not ontologically.[23] In our context, the operation of the "spirit" is necessary to animate the embryo. If the vagina is too long, impregnation may be impeded by the fact that the "spirit" of the semen has evaporated en route.[24]

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[19] *De gen. animal.* 772b 13–17.

[20] *Ibid.* 773a.1–3.

[21] Albert's *De animalibus* is a running commentary on Aristotle's biological corpus. Books 1–10 are a paraphrase of Aristotle's *Historia animalium,* books 11–14 of Aristotle's *De partibus animalium,* books 15–19 of Aristotle's *De generatione animalium.* Books 20–26 are Albert's own additions. Aristotle's biological treatises were, before 1220, translated from the Arabic into Latin by Michael Scot and, in 1260, from the Greek into Latin by William of Moerbeke. Albert used the Scot translation. For the history of the Latin Aristotle, see Bernard Dod, "Aristoteles Latinus," in *The Cambridge History of Later Medieval Philosophy,* ed. Norman Kretzmann, Anthony Kenny, and Jan Pinborg (Cambridge: Cambridge University Press, 1982), pp. 58–59, 63, 67.

We also possess Albertus Magnus, *Quaestiones super de animalibus,* ed. Ephrem Filthaut in *Opera omnia,* 12 (Münster: Aschendorff, 1951–). In this work no mention is made of Albert's theory of twins. The work is a report (*reportatio*) made by Conrad of Austria and probably was not authorized by Albert.

[22] Of course Albert was also familiar with Aristotle's theory of superfoetation. According to this theory the generation of twins is due to successive conceptions (*impregnari una impregnatione post aliam*). Cf. *De animal.* 9.1.5. As I have already indicated, superfoetation is only one aspect of the explanation of the generation of twins.

The three other factors which contribute to the separation of generative matter are brought about by the female. The first is analogous to the cause of the division of the male sperm. The female liquid, which the physicians call *sperma mulieris,* is also sent out in successive parcels By "female sperm," Albert probably means the vaginal secretion. Note that Albert, in contradistinction to Aristotle, is of the opinion that the female does contribute "semen" to the embryo, although this semen has no formative virtue.[25]

Secondly, the division is caused by the way the male sperm is "sucked in" by the uterus (*gluttitio matricis*). Sometimes the male sperm is taken in by the womb in successive turns, and then each quantity of sperm absorbed may reach a different part of the womb.

Finally, the sperm (here in the sense of the male and female contributions to the zygote) may be divided by the way the female, during coitus, moves her uterus for her delectation. Albert informs us that the causes of enjoyment of the female during sexual intercourse are threefold: the secretion of the female sperm (*projectio*); the absorption of the male semen (*gluttitio*); and, finally, the movements of the womb. This last kind of enjoyment is induced by the fact that the glans penis (*collum vesicae*) is grasped by the womb (*vulva*).

It is remarkable that the *projectio* and the *gluttitio,* which are here enumerated among the sources of sexual pleasure for the female, are precisely the two other causes of the division of the *sperma* (zygote). So, to summarize Albert's position, it is mainly due to the delectation of the female that the division of the sperm, that is, of the generative material, and thus the generation of twins, is brought about.

Other passages in *De animalibus* give the impression that Albert has developed a theory of "the mechanism" of sexual delight for the female. Albert relates the female delectation during intercourse with the anatomical structure of the vagina. The vagina consists of muscular tissue (*caro nervosa*), and for this reason it is perceptive to delight (*sensibilis dilectationis*).[26] The delight is induced by the fact that the sinews of the womb are prickled by the sperm (male and female) which is poured over them.[27] The state of sexual arousal may even reach such a level that the accompanying movements of the womb tear loose a fetus from its roots and membranes. At the moment that the womb opens itself to "suck in" (*sugit*) the new sperm, the former fetus is aborted.[28] Here the movements of the womb are again associated with female sexual delight.

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[23] See James Bono, "Medical spirits and the medieval language of life," *Traditio,* 1984, *40* 119–26, for an extensive discussion of Albert's notion of spiritus and its historical background See also Demaitre and Travill, "Human Embryology," pp 422–24, for some observations on Albert's theory of animation within the confines of embryology

[24] *De animal* 16.2.1 and 2.

[25] See also Demaitre and Travill, "Human Embryology," pp 417–19, for other passages in the work of Albert which suggest that Albert distinguishes components in the female contribution which Aristotle had left undifferentiated as the catamenia

Before turning to the sources of Albert's theory of the generation of twins in *De animalibus* 9.2.6, I shall elaborate on its philosophical background. We have already noted that, according to the Aristotelian theory, twins fall within the category of monsters· they are something out of the ordinary Twins are the result of a corruption of a natural process. The tendency, already perceptible in Aristotle, to attribute this corruption to the matter which is involved in the generation of twins becomes very distinct in Albert's theory. The fact that Albert, in his embryological theory, works out this particular aspect is bound to be illuminated by a discussion of his theory of monsters in general.

The most profound exposition on monsters may be found in Albert's commentary on book 2 of Aristotle's *Physics* Book 2 contains, among other things, the well-known discussion concerning whether Nature acts for an end. One of the "proofs" that Nature does act for a purpose is the fact that otherwise one could not speak about the faults of Nature: "For, everything which happens in the correct way, possesses this correctness with regard to an end it is due to accomplish, and when it fails, its failure is attributable to the fact that, due to the corruption of a natural principle, it has not accomplished its end." The failures of natural processes which are for a purpose but which do not accomplish this purpose are in this context considered monsters.[29]

After presenting this definition, Albert surveys the causes of the failure of a natural process. He distinguishes four main categories of causes—and subdivides each—all of which have something to do with the disposition of the matter on which an agent acts. Diminution of matter (*materiae diminutio*), abundance of matter (*materiae superfluitas*), disproportion of matter with regard to the qualities conveyed upon it (*qualitatum disproportionabilitas ad materiam*), and a deficiency of the container (*continentis malitia*) —all may prevent the achievement of the purpose of a natural process.

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[26] *De animal* 2 2 4 Albert's observations on the intricacies of female sexual delight have no counterpart in the text of Aristotle Cf *Hist animal* 3 510b 13

[27] *De animal* 9 1 5 This information is not to be found in Aristotle Cf *Hist animal* 3 510b 13

[28] *De animal* 9 1 5 This passage also has no counterpart in Aristotle

[29] Albertus Magnus, *Physica* 2 3 3 , *Opera omnia,* 3, ed A. Borgnet (Paris Vives, 1890–99), p 166 "Similiter utique fiet in physicis quia quidquid ibi recte fit, non habet rectitudinem, nisi ex proportione ad finem debitum quem attingit et cum peccat, peccatum suum perpenditur ex hoc quod finem illum non attingit propter corruptionem alicuius principii naturalis Monstra enim sunt peccatum illius quod in natura est propter aliquid." On p 168 "Omnia enim haec monstra peccata naturae sunt intendentis finem aliquem quem attingere non valet propter corruptionem alicuius principii naturalis "

In his discussion of the category "monsters due to abundance—in number and in magnitude—of matter," we encounter Albert's theory of twins as an illustration. Abundance and division of sperm in the womb causes the birth of twins. The division of the sperm, that is, the matter from which the product of conception is made, is induced by the movements made by the female during mating. Albert adds that if the division of the sperm is not completed, the twins will be conjoined.[30] Albert's commentary on the *Physics* illustrates the place of his theory of twins within the broader framework of his theory of teratogenesis.

It is important to stress that Albert considers monsters to be phenomena which belong to natural philosophy: they have a natural explanation. His attitude toward abnormal births resembles very much the reflections of Francis Bacon on the study of monsters. Bacon gave the study of monsters a place within his program for the reform of human knowledge, developed in *The Advancement of Learning*.[31]

The idea that the corruption of a natural process is caused by the matter involved recurs in other works of Albert's It is most boldly stated in *De animalibus* 18.1.6: "There is no cause of monstrosity other than matter" (*Causa monstruositatis non est nisi in materia*).[32] Although Aristotle also seems to blame matter for the generation of monsters, he does not develop the idea as fully as does Albert.[33] The extensive etiology of monsters discussed above is lacking in Aristotle's text of the *Physics*.

It is possible that Aristotle's belief that the male semen cannot be the cause of monstrosities induced Albert to develop his etiology of abnormal births in the way indicated above. In *De generatione animalium* 4.4 (769b.10–770a.10), Aristotle attacks Democritus's theory of teratogenesis. Aristotle informs us that Democritus attributes the generation of monstrosities to the fact that two emissions of semen meet. Not only embryos whose parts are grown together, but also man-headed calves and other things seem to be explained in this way by Democritus. Aristotle ripostes that abnormal births of the category where several species are mixed in one offspring can never be caused by a mixing of seminal fluids, because each species has its own period of gestation.

[30] Jennifer Lynn Calagan, "The conjoined twins born near Worms, 1495," *J Hist Med*, 1983, *38* 450–51, draws attention to what apparently is the earliest documented case of craniopagus twins (1495), a rare type of conjoined twins

[31] See Katharine Park and Lorraine Daston, "Unnatural conceptions the study of monsters in sixteenth- and seventeenth-century France and England," *Past and Present*, 1981, *92* 20–54, for a discussion of Bacon's notion of monsters and its (modern) intellectual context.

[32] Cf also *Commentarii in 1–4 Sententiarum* 2. dist.18G art.5, in *Opera omnia*, 27, ed A. Borgnet and *Quaestiones de animalibus* 18, quaestiones 5 and 6 A good discussion of the astrological explanation of abnormal births in the works of Albert the Great is presented in Demaitre and Travill, "Human Embryology," pp 434–37 The role of matter in the generation of aberrant offspring is, unfortunately, not discussed by them In my opinion, matter is the most important factor in Albert's theory of teratogenesis

[33] Cf *De gen. animal.* 4 770a.5–10 and *Metaph* 6 1027a 10–15

It is remarkable that Albert, in all those passages where he insists that the matter is responsible for monstrosities, cites with approval this opinion of Aristotle.[34] The purport of the citations seems to be that a mixing of semen (*commixtio spermatum*) from animals of different species is not possible, and that for this reason the male semen alone cannot be the cause of an abnormal birth.

Perhaps it is useful at this point to compare this notion of monsters with the idea of monsters which was dominant in seventeenth-century New England. In a very interesting article which treats homosexuality and bestiality in Puritan New England, the author examines, among other things, two cases where people were executed on the evidence of buggery.[35] In both cases the evidence consisted of a deformed fetus of an animal, "a prodigious monster." Unfortunately, some people saw a resemblance between the fetuses and the poor suspects. Apparently the Puritans believed that it was possible for a man to impregnate an animal. This opinion was not shared by Albert.[36]

Albert's theory of monstrous births clearly forms the backbone of his theory of twins. I shall now return to the latter, and shall discuss the question of its sources, especially those developed in *De animalibus* 9.2.6. We already observed that Albert's source certainly was not Aristotle, for Aristotle remains silent regarding the causes of the division of the matter, which is fundamental to the process of generating twins. Are Albert's observations on the "technical" sexual details of the generation of twins derived from some other source, or are they another instance of his much-appreciated empirical approach?[37]

I think the former is the case. From the footnotes of Stadler's edition of *De animalibus,* it is abundantly clear that Albert constantly used Avicenna's *De natura animalium* and the *Liber Canonis.* According to one expert in the field of medieval medical learning, Albert was one of the pioneers in the Latin West in the use of Avicenna as a medical authority.[38] Although Albert, in his digression on twins, does not refer to Avicenna (neither does Stadler), it is highly probable that he derived the essentials of his theory of twins from Avicenna. The *De natura animalium* and the *Liber Canonis* contain remarks on the generation of twins which are very similar to those of

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[34] Cf *Phys.* 2 3, p 168, *Sent* 2 dist.7 F art.9, p 158, *De animal* 18 1 6

[35] Robert Oaks, " 'Things fearful to name' sodomy and buggery in seventeenth-century New England," *J Soc Hist*, 1978, *12* 268–81

[36] Oaks assumes, however incorrectly, that the Puritans, with their man-like creatures, were not far removed from the Middle Ages I hope that my discussion of teratogenesis has made it clear that, on this evidence, these two people would never have been accused of buggery in the Middle Ages

[37] James Rochester Shaw, "Albertus Magnus and the Rise of an Empirical Approach in Medieval Philosophy and Science," in *By Things Seen Reference and Recognition in Medieval Thought,* ed. David Jeffrey (Ottawa University of Ottawa Press, 1979), discusses some aspects of Albertus Magnus's empirical approach to sexual anatomy and physiology This contribution is an almost verbatim reproduction of James Rochester Shaw, "Scientific empiricism in the Middle Ages Albertus Magnus on sexual anatomy and physiology," *Clio Medica,* 1975, *10* 53–64

[38] This is the opinion of Siraisi, "Medical Learning," pp 392–93

Albert. Thus, I doubt that Albert's observations stem from his own experience. Rather, this is another instance of Albert's use of Avicenna.

Like Aristotle, Avicenna is of the opinion that the overabundance of sperm, and the apportionment of this sperm—taken here in the sense of the mixture of the male and the female contributions to the product of conception—over several parts of the uterus, is decisive for the process of generating twins. He also enumerates the same factors for the division which Albert does: on the part of the male, the distinct impulses of the semen (*diversi impulsus spermati*), and on the part of the female, the three circumstances which contribute to her sexual delight; that is, the ejaculations (*eiectiones spermatis*), the movements of the orifice of the womb when it draws in the semen (*motus orificii matricis in sugendo*), and the movements of the womb (*motus matricis*).[39] We may safely assume that Albert in his theory of twins relied on *De natura animalium* rather than on the *Liber Canonis.*

I hope this paper has illuminated not only a medieval theory of the generation of twins, but also its philosophical context. The philosophical background essentially consisted of a general theory of teratogenesis, into which the embryological theory of the generation of twins was made to fit. Albert seemingly assimilated Arabic sources in order to develop Aristotle's theory. The way Albert fashions the remarks of Avicenna into a systematic exposition is striking.

Albertus Magnus's theory of the generation of twins did not remain untouched. It was undoubtedly taken up by the philosopher John of Jandun (1285/89–1328), and further research may uncover more followers of Albert on this point.[40] In his commentary on book 2 of the *Physics,* once again in connection with the discussion of whether Nature acts with a purpose, John of Jandun refers to Albert's theory of twins. Like Albert, he also remarks that sometimes the division of the sperm is not completed, in which case a creature with two heads or four limbs will result. Anxiety that such a terrible thing might happen probably induced him to add the following personal note: "Therefore it is dangerous that the female moves during sexual intercourse in the way prostitutes are reputed to do, because if at such moment they would conceive, they could generate an awful two-headed monster."[41]

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[39] Avicenna, *Liber Canonis* (Venice, 1508, reprint, Hildesheim Georg Olms, 1964), f 362, and Avicenna, *De natura animalium* (Opera Philosophica) (Venice, 1508; reprint, Louvain· Edition de la bibliothèque SJ, 1961)

[40] Another author who may have been influenced by Albert is Giles of Rome See, for this suggestion, Michael McVaugh in his review of Anthony Hewson's *Giles of Rome* in *Speculum,* 1977, *52* 987–89 Unfortunately, the discussion which Hewson (*Giles of Rome,* p. 180) devotes to this particular aspect of embryology is too superficial to provide a definite answer It is also possible that Giles derived his knowledge directly from Avicenna.

[41] John of Jandun, *Quaestiones super octo libros Physicorum Aristotelis* (Venice, 1551 reprint, Frankfurt Minerva, 1969), 2 q 13 f 39rb "Ideo periculosum est et inconveniens quod mulier in coitu agitet se et moveatur, sicut dicuntur facere mulieres meretrices, quia, si tunc conciperent, posset generari monstrum bicorporeum et valde turpe et horribile"

GRADUATE EDUCATION IN THE HISTORY OF MEDICINE: GREAT BRITAIN

Compiled by Christopher Lawrence

In the 1986 spring issue of this journal, Ann G Carmichael and Ronald L. Numbers surveyed "Graduate education in the history of medicine. North America" (*Bull Hist Med*, 1986, *60*. 88–97) British graduate (called post-graduate) education differs considerably from the American model and is also extremely variable in the extent of teaching and supervision a student can expect. In general, admission to a Ph D (D.Phil. at Oxford) usually requires a master's degree (M.A., M Sc, M.Litt, M.Phil.), in some historically related discipline. These courses usually last one year and may include research work An M.Phil. course usually lasts two years and always includes research work. Transfer from an M.Phil. to a Ph D is often possible. Some institutions offer these courses on a part-time basis It is usually at the level of a master's degree that formal training in the history of science or medicine is obtained. Institutions will often waive the requirement of a master's degree in the light of evidence of familiarity with the history of medicine or science (obtained, for instance, at undergraduate level) The British Ph D., generally, is purely a research degree, the student mostly working with one or sometimes two supervisors.

The following list comprises institutions offering Ph D supervision in the history of medicine, and those offering a relevant master's degree Obtaining a master's degree at an institution does not, of course, oblige a student to continue with the Ph.D. at the same institution The list is by no means comprehensive, there are many individuals in departments (of history or sociology, for example) willing to supervise Ph.D. students A convenient guide to such departments is. *A List of Theses and Dissertations in the History of Science in British Universities in Progress or Recently Completed,* obtainable from The Executive Secretary, The British Society for the History of Science, Whitehill Cottage, Old Whitehill, Tackley, Oxford OX5 3AB, U.K., $4 post paid

The main centres for the study of the history of medicine in Great Britain are the four Wellcome Units and the Wellcome Institute The order below is alphabetical, by university, and lists the faculty in the relevant departments qualified to supervise Ph.D s and those who are usually involved in the teaching of any master's degrees available. The historians who are included without a designated provenance are members of departments who are not primarily involved in the history of medicine but might well be involved in master's teaching or in joint supervision Associated staff refers to members of other departments or persons not on the full-time teaching staff but who are working on the history of medicine in association with the department concerned. In all cases the following details have been provided by the department, not the compiler of the survey

Bull Hist Med 1987 61 247–252

UNIVERSITY OF ABERDEEN: Department of History and Philosophy of Science, Kings College, Aberdeen AB9 2UB M.Sc., M.Litt., courses in history of science and medicine, Ph D.
Andrew Wear, Ph.D
history of medicine, especially Renaissance anatomy and medicine, early modern social history of medicine
Nicholas W. Fisher, Ph.D.
A. G Molland, Ph D

THE QUEEN'S UNIVERSITY OF BELFAST: Department of History and Philosophy of Science, Belfast BT7 1NN, Northern Ireland. No taught master's degree, M.A. (by thesis), Ph.D.
W. A. Gabbey, Ph D.
Renaissance and 17th-century medicine
R. E. Hall, M.A.
Graeco-Roman antiquity; Islamic and Western Middle Ages; areas of the Byzantine Middle Ages, the Renaissance and first half of the 17th century.

UNIVERSITY OF CAMBRIDGE: Department of the History and Philosophy of Science, Free School Lane, Cambridge CB2 3RH. M Phil (one year), Ph.D.
John Forrester, Ph D.
history of psychoanalysis, history of the human sciences
Simon Schaffer, Ph D
history of experimental science and medicine since 1600
J. A. Bennett, Ph.D.
N. Jardine, Ph.D.
Associated Staff
Geoffrey Lloyd, Ph.D.
classical medicine

Wellcome Unit for the History of Medicine (The Wellcome Unit is part of the Department of the History and Philosophy of Science.) M.Phil. (one year), Ph.D.
Andrew Cunningham, Ph.D.
history of medicine since 1600
Roger French, D.Phil.
history of ancient, medieval, and Renaissance medicine
Associated Staff
Perry Williams, Ph D.
19th-century British medical professions
Adrian Wilson, Ph.D.
medicine, midwifery and childbirth in the early modern period

UNIVERSITY OF EDINBURGH: Science Studies Unit, 34 Buccleuch Place, Edinburgh EH8 9JT. Ph.D.
John Henry, Ph D.
17th-century science and medicine, especially the relations of science, magic, and religion

Steven Shapin, Ph D.
history of experimental science and medicine, history of phrenology
Barry Barnes, Ph D.
David Bloor, Ph.D.
David Edge, Ph.D

UNIVERSITY OF ESSEX: Department of History, Wivenhoe Park, Colchester CO4 3SQ. M.A. (History and philosophy of the social and political sciences), Ph.D.
Ludmilla Jordanova, Ph D.
medicine since 1700; women and medicine, French medicine; medical theory and medicine in culture, comparative approaches
Department of Sociology
Joan Busfield, M.A.
history of psychiatry

UNIVERSITY OF GLASGOW: Wellcome Unit for the History of Medicine, 5 University Gardens, Glasgow G12 8QQ. Ph D.
Marguerite Dupree, D.Phil
family and demographic history; history of hospitals
Stephen Jacyna, Ph D
history of neurophysiology and cell theory, history of clinical pathology
David Hamilton, Ph D.
history of surgery
Associated Staff
M. A. Crowther, Ph D
history of forensic medicine
Derek Dow, Ph D.
history of hospitals

UNIVERSITY OF KENT AT CANTERBURY: Unit for the History, Philosophy and Social Relations of Science, Physics Laboratory, Canterbury, Kent CT2 7NR. M.A., M Sc. (History of science), Ph D.
Maurice Crosland, Ph D
history of French science and medicine
Alec Dolby, A.M
history of psychology
Crosbie Smith, Ph D
Associated Staff
Stewart Richards, Ph.D.
history of physiology, vivisection

UNIVERSITY OF LANCASTER: Department of History, Lancaster LA1 4YG. M.A. (History of science), M.Phil., Ph.D.
John Brooke, Ph D.
biochemistry in relation to medicine
Roger Smith, Ph D.
19th-century biomedical sciences, especially neurophysiology, psychology, psychiatry, and medicolegal issues
Robert Fox, Ph.D
Peter Harman, Ph.D.

UNIVERSITY OF LEEDS: Division of History and Philosophy of Science, Department of Philosophy, Leeds LS2 9JT Diploma in history and philosophy of science, M.Phil. (by research), Ph D
J. R. R. Christie, M.A.
  intellectual and institutional history of 18th-century medicine
M. J. S. Hodge, Ph.D.
  medical and physiological biology in relation to evolutionary theory
R. C Olby, D.Phil
  biochemical and genetical resources, their development, funding and applications
G N. Cantor, Ph.D.

UNIVERSITY OF LEICESTER: Departments of History and Sociology, University Road, Leicester LE1 7RH. M.A. (Victorian studies), Ph.D
W H. Brock, Ph.D.
  history of biochemistry
S. F. W Holloway, B.A.
  social history of 19th-century medicine
N. D. Jewson, M.A.
  social history of doctor/patient relations
A. G Keller, Ph D
  Renaissance medicine
I. H Waddington, B.A.
  social history of 18th- and 19th-century medicine

UNIVERSITY COLLEGE LONDON: Department of the History and Philosophy of Science, Gower Street, London WC1E 6BT. M Sc (taught jointly with Wellcome Institute staff), M.Phil., Ph D.
P. M. Rattansi, Ph.D.
  medicine in 17th-century England, Paracelsianism
Associated Staff
W. A. Smeaton, D.Sc
  18th-century medicine and chemistry

UNIVERSITY COLLEGE LONDON: The Wellcome Institute for the History of Medicine, 183 Euston Road, London NW1 2BP. M.Sc (taught jointly with Department of History and Philosophy of Science), M.Phil , Ph.D
Janet Browne, Ph.D.
  botany and medicine; generation theory
William Bynum, Ph.D.
  medicine since 1700; history of psychiatry; anthropology
Lawrence Conrad, Ph.D.
  Arabic/Islamic medicine
Lindsay Granshaw, Ph D.
  history of hospitals; surgery, nursing; modern medicine and society
Christopher Lawrence, Ph.D.
  medicine since 1700
Michael Neve, Ph.D.
  provincial science and medicine, history of psychiatry; medicine and biology, degeneration

Vivian Nutton, Ph.D
  classical, medieval, and Renaissance medicine
Richard Palmer, Ph D.
  Italian Renaissance medicine, plague
Roy Porter, Ph D
  18th-century social history of medicine, history of psychiatry; unorthodox medicine
Associated Staff
Nigel Allen, Ph D.
  biblical studies and medicine
G M Prescott, Ph.D
  19th-century art, photography, and medicine
William Schupbach, Ph.D
  iconography of medical history
Dominic Wujastic, D.Phil
  Sanskrit literature and medicine; Indian medicine

UNIVERSITY OF MANCHESTER: Wellcome Unit for the History of Medicine, and Centre for the History of Science, Technology and Medicine, Department of Science and Technology Policy, The University, Manchester M13 9PL M Sc , Ph.D
Roger Cooter, Ph D
  science in historical cultures, popular medicine; transformations in 20th-century medicine, especially orthopaedics
Jonathan Harwood, Ph D
  history of genetics and 20th-century biology, especially in Germany, history of psychology
John Pickstone, Ph D
  history of biomedical sciences; social history of medicine in industrial England
E J Yoxen, Ph.D
  history of biotechnology and molecular biology; recent medical technology, ethical issues in modern medicine
Associated Staff
David Barker, B.A
  eugenics and social policy, mental handicap
Stella V F Butler, Ph.D
  medical science and education, medical technology, Manchester science and medicine
W Luckin, M Sc
  history of epidemiology and demography, history of pollution
M. Worboys, Ph.D.
  clinical science and medicine; biomedical sciences, 1880–1914

THE OPEN UNIVERSITY: Department of History of Science and Technology, Walton Hall, Milton Keynes MK7 6AA. M Phil , Ph.D (Students can obtain an Open University Ph.D. and be supervised by someone from another institution.)
Noel Coley, Ph.D.
  medical chemistry
Colin Russell, Ph D.

UNIVERSITY OF OXFORD: Wellcome Unit for the History of Medicine, 45–47 Banbury Road, Oxford OX2 6PE M.Litt., M.Phil, D.Phil.
Margaret Pelling, M.Litt
early modern medical practice in England; 19th-century British public health
Charles Webster, D.Sc.
early modern science and medicine; health services in the 20th century
Paul Weindling, Ph D.
19th- and 20th-century biology, medicine and public health, Germany and Britain
Associated Staff
Joan Austoker, Ph.D.
20th-century biochemistry, cancer research, 20th-century eugenics in Britain
Irvine Loudon, D.M.
medical care in Britain, 1750 to present, obstetrics in Britain, 1750 to present
T.J. S. Patterson, D.M.
plastic surgery; European medicine in India

THE UNIVERSITY OF SHEFFIELD: Department of Economic and Social History, Sheffield S10 2TN Ph.D.
John Woodward, D.Phil.
social history of medicine since 1700; institutional history, occupational development within medicine; historical demography

MEDICO-HISTORICAL NEWS AND ACTIVITIES

CORRESPONDENCE AND REPORTS

IN MEMORIAM: D. V. SUBBA REDDY (1899–1987)

Professor D. V. Subba Reddy, a distinguished pioneer of medical history in India, died on 18 February 1987 at the age of eighty-seven. His death is a sad loss to the field in India and elsewhere.

Professor Reddy graduated from Madras Medical College and began his career as a professor of physiology. He soon developed an interest in the history of medicine under the influence of Henry E. Sigerist, then director of the Johns Hopkins Institute of the History of Medicine. He contributed a number of articles to the *Bulletin of the History of Medicine* during the thirties and forties.

Professor Reddy founded the first department of the history of medicine in India in 1956. The department soon moved to its present site at Osmania Medical College, Hyderabad, and Professor Reddy became its director. He also founded the Indian Association of the History of Medicine and under its auspices from 1956 to 1962 edited the *Indian Journal of the History of Medicine,* which played an important role in inspiring medical scientists to recognize and study the ancient medical heritage of India.

The department at Osmania College eventually evolved into the present-day Indian Institute of History of Medicine. As director of the Institute, Professor Reddy built up a substantial library that was unique not only in India, but in the whole of Southeast Asia. Professor Reddy also organized a medical history museum in the Institute and contributed to it many artifacts he had personally collected from medical, non-medical, archeological and epigraphical sources. He was founding editor of the *Bulletin of the Indian Institute of History of Medicine,* which began publication in 1963, and served in this post for some ten years.

Professor Reddy's research work in the history of medicine resulted in some twenty monographs and numerous articles. His books included *Antiquity of Syphilis (Venereal Diseases) in India* (1936) and *Glimpses of Health and Medicine in the Mauryan Empire* (1966).

Professor Reddy was active in representing India, and on occasion, Asia, at international history of medicine congresses, and was elected a member of the International Academy of History of Medicine. He was also associated with the National Commission for History of Sciences in India. In retirement, Professor Reddy started an association for care of the aged, with the goal of building a retirement home for the elderly.

Professor Reddy's name has become synonymous with history of medicine in India. It can be said without any hesitation that he was the inspiration behind all research workers and scholars of history of medicine in India, and it is interesting to note that a student of Marielene Putscher of the University of Cologne came to the Institute to write a thesis on Professor Reddy and the Institute. It was published in German in 1985.

B. RAMA RAO

ANNOUNCEMENTS

NATIONAL NEWS

American Institute of the History of Pharmacy. Members of the Institute gathered in Chicago for their annual meeting, 28–30 March 1987. The meeting included a symposium on "The Pharmaceutical Survey Revisited."

A new bibliography, *A Selection of Primary Sources for the History of Pharmacy in the United States: Books and Trade Catalogs from the Colonial Period to 1940,* has been published by the Institute. Compiled by Nydia M. King, the book provides a list of eighty-nine works and brief descriptive essays of each one. Nearly all of the works described are available in microfiche or hardbound facsimile editions from University Microfilms International. The bibliography can be obtained at a cost of $10.00 from the Institute, Pharmacy Building, 425 N. Charter Street, Madison, Wisconsin 53706.

Collaborators Wanted. J. Z. Fullmer of the Ohio State University invites interested scholars to join a small British-American ad hoc group formed to produce a "Readers' Guide" to Abraham Rees's *Cyclopaedia.* The proposed guide to this multivolume nineteenth-century work will in all likelihood include biographical notes on its contributors; a concordance to the major articles; an analytical subject index; and chapters on the printing and publishing history of both the English and American editions. Because the *Cyclopaedia* is inclusive, collaborators from many fields, including all the humanities, the sciences, medicine, the arts, and technology, can be accommodated in areas coinciding with their scholarly interests. For further details, please write to Professor J. Z. Fullmer, Department of History, Dulles Hall, Ohio State University, Columbus, Ohio 43210.

College of Physicians of Philadelphia. The Historical Collections of the Library of the College of Physicians are extending their coverage of printed materials to include those from 1900 to 1965. This expansion is due in part to present research trends in the history of twentieth-century medicine. The Historical Collections also contain the manuscript collection, photographs and prints, and archives of the College. Further information on the collection may be obtained by writing to Mr. Thomas A. Horrocks, Curator, College of Physicians of Philadelphia, 19 South 22nd Street, Philadelphia, Pennsylvania 19103.

Columbia University. The Health Sciences Library's 1986–87 series of lectures included: 9 October 1986, "Confessions of a Museum Addict," Roderick Calverley; 6 December 1986, "The Entry of Blacks and Other Minority Students into U S Medical Schools," Steven Shea; 26 March 1987, "Reminiscence and Perspective," Paul A Marks, and 15 April 1987, "New Orleans Surgeons of the Early Nineteenth Century," Gustavo A. Colon

Cornell University. A symposium dedicated to the memory of Henry Guerlac was held at the University from 16–18 April 1987 Entitled "Analyzing the Inchoate: Complex Interrelations in the Humanities and the Sciences," the conference was sponsored by the Society for the Humanities, the Western Societies Program, and the Program in the History and Philosophy of Science and Technology, all of Cornell University

Health Laboratories. Between 15 May and mid-October, 1987, an exhibit on "The New Age of Health Laboratories, 1885–1915" will be displayed at the National Library of Medicine. The exhibit marks the joint centennials of the National Institutes of Health and the Pasteur Institute An illustrated brochure on the exhibit is available at no charge from the Chief, History of Medicine Division, National Library of Medicine, Bethesda, Maryland 20894.

History and Philosophy of Medical Genetics—Call for Papers. The historical and philosophical problems of medical genetics will be explored in the twenty-seventh Transdisciplinary Symposium on Philosophy and Medicine, which will take place in San Francisco on 11–12 December 1987 Papers that discuss the concepts of genetic health and disease, the nature of explanation in medical genetics, the ethics and ethos of medical genetic research and practice from the perspectives of history and philosophy of science and medicine, medical anthropology or sociology, and bioethics are invited Papers will be competitively reviewed and plans are being made to publish the conference papers in the philosophy and medicine series of the D. Reidel publishing company. Scholars interested in presenting a paper should request details from Dr. Eric T Juengst, Division of Medical Ethics, UCSF School of Medicine, 1362 Third Avenue, San Francisco, California 94134. The deadline for submissions is *1 July 1987.*

History of Physiology. Among the various symposia at the seventy-first annual meeting of the Federation of American Societies for Experimental Biology in Washington, D.C., 29 March–2 April 1987, was one entitled "The Five Founders of the American Physiology Society" Speakers included Toby Appel, "The Founding of the American Physiological Society", A. Clifford Barger, "Bowditch, the First President of the American Physiological Society"; W Bruce Fye, "Martin, the First Secretary and Treasurer of the American Physiological Society", S Ochs, "Mitchell, the Second President of the American Physiological Society"; J Hoffman, "Chittenden, the Third President of the American Physiological Society"; and Gerald L Geison, "Curtis, the Host of the Organizational Meeting of the American Physiological Society." D L Gilbert was the chairman of the session.

Johns Hopkins Institute of the History of Medicine. Presentations in the spring 1987 colloquium series included: 5 February, "Pietist Medicine in Pre-Revolutionary

America," Renate Wilson (Johns Hopkins University), 12 February, "Philadelphia Public Health in the Age of Scientific Medicine," Edward T. Morman (University of Pennsylvania); 19 February, "Assimilating Statistics to Medicine: Call Me When the Revolution Starts," Harry Marks (Wood Institute of the History of Medicine); 26 February, "Surgery Run Amok· Chronic Appendicitis as a Disease Entity," Dale C. Smith (USUHS School of Medicine); 19 March, "Creating the NIH Grants Program, 1937–46," Daniel M Fox (SUNY at Stony Brook); 9 April, "Translating Ancient Chinese Medical Literature: History and Current Issues," Paul U. Unschuld (Ludwig Maximilians Universität, Munich), 16 April, "Coming into the Forefront: Theoretical and Applied Immunity in the 1930s," Anne-Marie Moulin (Chamalière, France); and 23 April, "Comparative Approaches to Medical Education in the Nineteenth Century: Britain, Germany, and the United States," Thomas N Bonner (National Library of Medicine).

Johns Hopkins Medical Institutions. The spring humanities program, sponsored by the Committee on Cultural and Social Affairs, was concerned with "Illness and Healing: Humanistic Approaches to the History of Therapy." The program included seminars throughout the spring semester on the following topics: "Consent and the Body: Injury, Departure and Desire," Elaine Scarry, "Reversible Boundaries and Explosions of Force: The Greek Theater and Festival as Female Therapy," Barbara Gold; "Therapy—Modern and Post-Modern· Borges, Camus, Bruno Schulz, and Wittgenstein," Henry Sussman, "From *Pharmakos* to *Pharmakon.* The Therapeutic Dimensions of Myth and Ritual," René Girard, "Psychotherapy and Society," Jerome Frank; and "Medical Therapy for the Future," Donald Coffey. The series was concluded on 26 April with a half-day colloquium featuring the following speakers· Paul Talalay, "From Medicine Man to Man-Made Medicine", Robert Hudson, "The Relationship of Theory to Therapy in Medical Practice"; Mary Poovey, " 'Scenes of an Indelicate Character': The Medical 'Treatment' of Victorian Women"; and Elizabeth Fee, "Public Health as Social Therapy." The Medicine and Humanities series is funded by the National Endowment for the Humanities.

Medical History—Armenia. The summer 1986 issue of the *Armenian Review* (vol 39, no 2) contains two papers on medical history topics The first is a general review of the history of medicine in ancient and medieval Armenia and the second is a biographical sketch of Amirdovlat Amasiatsi, a medieval Armenian scientist, linguist, and physician

Medical Humanities Review. This new journal, a twice-yearly review of publications in the medical humanities, made its debut in January 1987. It is edited by Ronald A. Carson and Thomas H. Murray, and produced by the Institute for the Medical Humanities of the University of Texas Medical Branch. Volume 1, Number 1 contained reviews of recent works focusing on ethics in medicine, political and social issues in medicine, theology and medicine, and social history of medicine. The journal intends to cover both American and overseas publications of interest to those in the medical humanities Subscriptions may be entered by sending $20 00 ($40 00 for institutions) to *Medical Humanities Review,* Institute for the Medical Humanities, University of Texas Medical Branch, Galveston, Texas 77550.

Museum of Ophthalmology. Those interested in the history of ophthalmology or related subjects should be aware of the resources available at the Museum of Ophthalmology of the American Academy of Ophthalmology. Located in San Francisco, the Museum has a collection of more than 3,000 items dating from the fourteenth century to the present, including a library of ophthalmological literature The Museum is open to the public, library and storage access is open by appointment only. For more information, write to Ms Susan Cronenwett, Director, Museum of Ophthalmology, Foundation of the American Academy of Ophthalmology, P.O. Box 6988, 655 Beach Street, Suite 300, San Francisco, California 94101-6988.

National Library of Medicine. "Women, Medicine, and Foreign Study, 1860–1915" was the title of a lecture given by the NLM Visiting Historical Scholar, Professor Thomas N Bonner, on 6 March 1987, at the National Library of Medicine The talk was sponsored by the History of Medicine Division

The History of Medicine Division's regular seminar series included two lectures by Professor Barbara M. Stafford in conjunction with a course on metaphor and medical theory that she is offering at the Folger Library Entitled "Knowing and Perceiving: Anatomy and Physiology" and "Problematics of Surface The Lessons of Microscopy," the lectures were given on 9 February and 23 March, respectively.

New York Hospital—Cornell Medical Center. Speakers in the History of Psychiatry Section's spring series included: Jeffrey Wollock, "Changing Concepts of the Psychology of Work in the Seventeenth and Eighteenth Centuries," 1 April; Eric T. Carlson, "Multiple Personality and Hypnosis: The First Century" and Jacques Quen, "Albert the Noorman and Albert the German, or, Colonial American Attitudes Toward the Neighbor Insane," 8 April; G. S. Rousseau, "Writing the History of Sexuality," 15 April, Doris Nagel, "Psychoanalytic Treatment on Trial (1929) A Fragment from an American Case Study," 29 April; Ralph Baker, "*Je Pense, Donc Vous Pensez,*" 27 May, and Fady Hajal, "Lovesickness A Major Health Hazard for Medieval Middle Eastern Youth," 10 June

Nuremberg Plus Forty: Health Ethics and Public Policy. The Humanistic Studies Program at the University of Illinois at Chicago is commemorating the fortieth anniversary of the Nuremberg Code, a landmark document providing the ethical foundations for human experimentation and medical research, by sponsoring a series of lectures, symposia, and exhibits As part of the program, a special exhibit and symposium were organized in the spring of 1987. "*Nicht Misshandeln Krankenhaus Moabit,* 1920, 1933, 1945" was on view from 23 March to 24 April 1987 at the Chicago Gallery of the University of Illinois The exhibit focuses on a municipal hospital in Berlin where many prominent German-Jewish physicians practiced, and which was disastrously affected by the Nazis' rise to power. After 1933, the Moabit hospital was transformed from an eminent teaching and research institution into a Nazi center for human medical experimentation. The exhibit is the result of the research and collecting efforts of Christian Pross, a Berlin physician and medical historian, and has previously been shown in Germany. The opening of the exhibit was accompanied by a symposium. Dr. Pross lectured on "The Moabit Experience National Politics and Medicine." This was followed by a panel discussion between Pross, Annette Friedman and Max Sampter on the "Personalities of Moabit" and the session ended with a lecture by Erich Loewy on "The Fate of Jewish Physicians During the Nazi Era"

Organization of American Historians. Members of the group convened for the eightieth annual meeting in Philadelphia from 2–5 April 1987 Sessions included "The Emergence of Mental Health Policy in Post-World War II America" with "The Politics of Mental Health in Post-World War II America," Gerald N Grob, and "William Menninger and Mental Health 'Salesmanship' in Post-World War II America," Lawrence J. Friedman (James T. Patterson, presiding; Nancy J. Tomes, commentator); "Cultural and Biological Dimensions of Behavior" with "The Emergence of a Modern Disease: From *Anorexia Mirabilis* to *Anorexia Nervosa,*" Joan Jacobs Brumberg; and "Smoking and American Culture," Allan Brandt (Judith Walzer Leavitt, presiding; Gert H. Brieger and Charles E Rosenberg, commentators); and "Boundaries of the Mind Psychiatry, Psychology and the Limits of Professional Power" with "Assessing the Minds of Children. Teachers, Doctors and Psychologists in Progressive America," Leila Zenderland; "On the Border Between Crime and Insanity," Janet Tighe, and "Assessing the Minds of Soldiers Military and Psychological Classification During World War I," Richard von Mayrhauser (John Burnham, presiding; Thomas Haskell, commentator).

UCLA Program in Medical Classics. Talks presented in the spring of 1987 in this series were the following· 10 February, "Pierre Fauchard (1678–1761)· The 'French Connection' Between Paré and Scientific Dentistry," Gorden Ting; 10 March, "Thomas Willis (1621–1675): Clinical Scientist," W Eugene Stern; 7 April, "Resistance to Innovation: The Community Health Center," Milton I. Roemer; and 12 May, "A Nobelist and His Laboratory Henry Dale (1875–1964) and the Idea of Chemical Transmission in the Nervous System," Frank C. MacIntosh The Medical Classics series has Robert G. Frank, Dora B Weiner, and Victoria L Steele as co-chairs.

U.S. Medical History Journals—Problems and Prospects. A seminar on this topic was sponsored by the History of Medicine Division of the National Library of Medicine on 25 February 1987. Panelists were editors Caroline Hannaway (*Bulletin of the History of Medicine*), Robert J. T. Joy (*Journal of the History of Medicine*), and Sharon Romm (*Medical Heritage*). The seminar was organized by James Cassedy

Uniformed Services University of the Health Sciences. Dale C. Smith, associate professor in the Section of Medical History at USUHS, recently received the Laurance D Redway Award for Excellence in Medical Writing from the Medical Society of the State of New York He was honored for his contribution entitled "A historical overview of the recognition of appendicitis" (*New York State Medical Journal,* 1986, *86* 571–83, 639–47)

University of Alabama, Reynolds Historical Library. The spring 1987 lecture series concluded with the following papers 9 April, "Paradigm of Reiter's Syndrome," Gene Ball, and 7 May, "Optometry—A Legal History," John G Classé

University of California, San Francisco. "Gender, Class and Race in the History of Health Sciences" was the topic of the spring quarter's lecture series Presentations included 31 March, "Overview of the History of Health Sciences, c. 1850–1950 Focus on Race, Class and Gender Issues in the U S ," Adele E. Clarke; 7 April, " 'One Part Cow and Nine Parts Devil': Pediatricians' Views of Wet Nurses in the Nineteenth Century," Janet Golden; 14 April, "Seeing What is Really There Representing Women in Renaissance Anatomy," Thomas Laqueur; 21 April, "Psychosurgery and Psychiatric Practice, c 1935–1955," Jack Pressman; 28 April, " 'How Much Training Do Medical Students Need in Experimental Sciences?'. A Nineteenth-Century Perspective," Arleen Tuchman, 5 May, "Gender, Disease and Psychiatric Diagnostics Jean-Martin Charcot and the Nineteenth-Century Idea of Masculine Hysterical Neurosis," Mark S Micale; 12 May, "Contraceptive Sterilization: Use and Abuse, 1889–1980," Adele E. Clarke; 19 May, "Asian Systems of Humoral Medicine," Charles Leslie, 26 May, "Women Physicians: Recent History, Prospects and Stresses," Lillian Cartwright. Adele E. Clarke served as coordinator of the series

University of Minnesota. The Owen Wangensteen Historical Library and the Program in the History of Science and Technology were the co-sponsors of a series of lectures in the spring quarter, 1987 Speakers were Anita Guerrini, "Natural History and the New World"; Jan McTavish, "It is the Very Model of a Modern Major Medicine: Aspirin and the Pharmaceutical Industry through World War I", Carol L Urness, "Peter Simon Pallas and the Natural History of Siberia in the Eighteenth Century", Arnold G. Frederickson, "A Botanical Detective Story· Where and When Did André Michaux Discover *Shortia galacifolia?*"; Thomas L Creed, "Going Public. Psychology in the 1920s"; Ted Greenfield, "An Inductivist Explanation of Evolution—Alpheus Packard's Neo-Lamarckian Theory"; John Beatty, "Evolution in the Atomic Age"; Michael A. Osborne, "Jean Boudin and Medical Geography in Nineteenth-Century France"; Sharon L Desborough, "The History of Potato Culture"; and Sheldon Hochheiser, "Pharmacological Standards and the Evolution of Food Color: Regulation in the United States."

Correction: In the note about the NLM microfiche catalogue (*Bull. Hist Med,* 1986, *60* 222–23), an incorrect Government Printing Office stock number was given The correct number is 017-052-00256-2.

PROGRAMS OF MEETINGS

Benjamin Waterhouse Medical History Society, Boston University School of Medicine

September 26, 1985 "No Magic Bullet," Allan Brandt

October 24, 1985 "Thoracic Surgery in Boston—Before the Cardiac Balloon Went Up," J. Gordon Scannell

November 21, 1985 "The Immigrant Midwives of Lawrence. The Conflict Between Law and Culture in Early Twentieth-Century Massachusetts," Eugene Declercq

January 16, 1986· "Boston Physicians in the Civil War," C Newton Peabody

February 13, 1986. "Dowries and Deaths in the Renaissance," Alan S. Morrison

March 13, 1986 Demonstration of selected items from the Warren Anatomical Museum, by David Gunner

September 11, 1986 "Appendicitis The Centennial of a Boston Surgical Breakthrough," John J. Byrne

October 2, 1986: "Public Pharmacology. How Nineteenth-Century 'Patent' Medicines Worked," J. Worth Estes

November 20, 1986 "Blood and Fat. Is the Reducing Diet a Relic of Medieval

Therapeutics?" William Bennett

February 26, 1987· "Hospitals and Medicine in Renaissance Florence," Katharine Park

April 9, 1987 "The Life and Work of an Eighteenth-Century Midwife," Laurel Thatcher Ulrich

May 7, 1987 "The Origins of the Stigma of Leprosy," Thomas Sabin

Johns Hopkins Medical History Club, Baltimore, Maryland

March 12, 1987: "The Transforming Principle," Maclyn McCarty

April 9, 1987. "Newly Discovered Letters of William Halsted and Harvey Cushing," Ira Rutkow

Mayo Foundation History of Medicine Society, Rochester, Minnesota

April 8, 1987. "Joseph Berkson, M D., Sc.D., and Mayo's Division of Biometry and Medical Statistics," Helen Golenzer and Leonard T. Kurland

Society of Medical History of Chicago

March 3, 1987. "History of Imbalming," Edward Jyvaskyla

April 7, 1987. "Complicity and Interdependence: The Unresolved Conflict in Modern German Medicine," Hartmut Hanauske-Abel

Washington Society for the History of Medicine, Bethesda, Maryland

March 26, 1987· "Ancient Arabic Surgery," Guido Majno; and "The Unwelcome Intruder," Sharon Romm

NEWS FROM ABROAD

Academy of Medicine, Toronto. The Museum of the History of Medicine, Academy of Medicine, Toronto, and the Mother-Child Project, Inc., have organized a travelling exhibit "Mother and Child *La mère et l'enfant,*" which will eventually be seen at eight museums across Canada and also in Milwaukee, Wisconsin. The exhibit centers on the history of childbirth, infant care, and motherhood in the Western tradition from 1600 to the present. More information may be had by writing to the Curator, Mrs Felicity Nowell-Smith, Museum of the History of Medicine, 288 Bloor Street West, Toronto, Ontario M5S 1V8, Canada.

Canada. McMaster University, in Hamilton, Ontario, is hosting the fortieth annual Canadian Learned Societies Conference from 24 May to 8 June 1987. Among the participating groups in the conference are the Canadian Society for the History of Medicine, the Canadian Society for the History and Philosophy of Science, and the Royal Society of Canada

France. The Société d'Histoire de la Pharmacie held its annual general assembly on 14 December 1986. The program included "Présentation d'ouvrages," Pierre Julien; "Louis Moissan: Un pharmacien trop tôt disparu," Claude Viel, "Comptes d'apothicaires normands," Olivier Lafont, and " 'Journal' de Maximilien Cagniole, apothicaire languedocien en 1879," Charles Cailhol.

History of Anesthesia. "No Laughing Matter: Historical Aspects of Anaesthesia" is the title of an exhibit currently mounted at the Wellcome Institute for the History of

Medicine, 183 Euston Road, London NW1 2BP, England. Held in conjunction with the Science Museum, and coinciding with the Second International Symposium on the History of Anesthesia at the Royal College of Surgeons, the exhibit will be on view from 8 June to 25 September 1987. A catalogue is available.

International Society for the History of Medicine. Information on the thirty-first international congress (Bologna, 30 August–4 September 1988) is now available. Those wishing details of the meeting should write to Organizing Secretariat, Conference Service srl, Via Tagliapietre 18/B, 40123 Bologna, Italy, *before 31 June 1987.*

Pasteur Institute Centennial. An international colloquium, "History of the Pasteur Institute," is being organized by members of the Institute as part of the scientific, historical and cultural activities marking its centennial. The colloquium will be held in Paris, 7–10 June 1988, and will deal with various aspects of the history of the Pasteur Institute and stress both scientific and institutional aspects of its development. Among the topics discussed will be the work of Louis Pasteur—contemporary evaluations, the Pasteur Institute as a model research institution, the relationships between the Institute and the pharmaceutical industry, medical schools and universities, the history of microbiology, immunology, biochemistry and chemotherapy at the Institute, and the French school of microbiology. Those giving papers include F Dagognet, Mirko Grmek, Gerald Geison, Jacques Léonard, Robert E. Kohler, Robert Olby, Harry W Paul, Claire Salomon-Bayet, and Jean Théodoridès Papers will be given in French and English.

Members of the scientific committee of the colloquium are François Jacob (president), F. Dagognet, Georges Duby, Mirko Grmek, A. Lwoff and E. Wolman For further information and for registration, write to Dr Michel Morange, IBM, Bâtiment Jacques Monod, Institut Pasteur, 25 rue du Dr Roux, 75015 Paris, France.

Society for the Social History of Medicine, Great Britain. The Society held a one-day conference concerning "The History of Medical Education" on 11 April 1987 The program was as follows "Provincial Medical Education in Eighteenth-Century England," Joan Lane; "Walking the Wards. Physicians', Surgeons', and Apothecaries' Pupils at the London Hospital, 1715–1815," Susan Lawrence, "Hampton Weekes Medical Student at St. Thomas's in 1802," John Ford; "German Medical Education Between *Kultur* and Professional Expertise," Paul Weindling; "Science versus Practice: Discussions on Medical Education in German Journals in the Second Half of the Nineteenth Century," Godelieve Van Heteren, "Medical Education for Women in the Late Nineteenth Century," Mary Ann Elston; and "The Politics of Nursing Education in Britain in the Inter-War Period," Anne Marie Rafferty.

University of Ottawa. The Hannah Chair in the History of Medicine at Ottawa sponsored a series of talks in the spring of 1987. Toby Gelfand delivered three lectures, the first an introductory one entitled "Healers in History", followed by "Barber-Surgeon. Healing and the Guilds" and "Health Officer: *Madame Bovary*'s Charles." Other speakers in the series were L M Eldredge, "Pain, Stars and Urine. Diagnosing Illness in the Middle Ages"; Rachel Maines, "Physical Therapies in Gynecology from Aretaeus to Freud"; Jacalyn Duffin, "Healer Accused, Toronto 1855", and Theodore Brown, "American Psychosomatic Passion, 1919–41."

Wellcome Institute for the History of Medicine. Among events at the Institute in the spring was a symposium on Brunonianism in Europe and America on 27 March Speakers were Christopher Lawrence, "Cullen, Brown and the Poverty of Essentialism"; Guenter Risse, "Brunonian Therapeutics New Wine in Old Bottles", Michael Barfoot, "Brunonianism Under the Bed. An Alternative to University Medicine at Edinburgh in the 1780s"; Nellie Tsouyopoulos, "The Influence of John Brown's Ideas in Germany"; Ramunas Kondratas, "The Brunonian Influence on the Medical Thought and Practice of Joseph Frank", and Roy Porter, "The Dangers of Excitement: Brunonian Psychiatry in England." On 27 April, Robert E. Kohler of the University of Pennsylvania delivered a public lecture entitled "Experimental Biology and the Rockefeller Foundation, 1930–50." Gert H. Brieger of the Johns Hopkins Institute of the History of Medicine is to direct a series of readings in American medicine in May and June.

The history of hospitals research seminar continued with presentations on British, European and American institutions Lectures included: 7 January, "French Hospitals in the Seventeenth Century," Colin Jones, 21 January, "A New Hospital for Women. The Transformation of the Manchester Lying-in Charity, c 1850," Joan Mottram, 4 February, "Modelling the Future Orthopaedic Hospitals and the Concept of Regionalization," Roger Cooter, 18 February, "Municipal General Hospital in Britain, 1918–39," Paul Etherington, 4 March, "The Hammersmith Hospital and Postgraduate Medical Education," James Calnan, 18 March, "How Long Fallen? A History of Homes for Unwed Mothers in America," Morris Vogel, 1 April, "The Edinburgh Infirmary in the Eighteenth Century: A Centre of Medical Enlightenment?" Michael Barfoot; 15 April, "The Management of Cancer in London Hospitals in the Nineteenth Century," Joan Austoker; 29 April, "Morally Reforming Madness: Thomas Mayo and the Treatment of Patients at Ticehurst Asylum, 1817–35," Charlotte Mackenzie; 13 May, "Concepts of Children's Hospitals from the Eighteenth to the Twentieth Centuries in Europe," Eduard Seidler; 27 May, "The Rothschild Hospital of Paris, 1852–1900," Toby Gelfand, and 10 June, "Labour, Health and Hospitals in America," David Rosner.

BOOK REVIEWS

RODERICK E McGREW with the collaboration of MARGARET P McGREW ***Encyclopedia of Medical History.*** New York McGraw-Hill Book Company, 1985 xiv + 400 pp $34 95

The contemporary dearth of up-to-date synopses dealing with the history of disease and medicine is painfully clear, especially when one plans to teach these subjects to college undergraduates and medical students Thus, any publication aiming at providing such an overview or bringing together information from the ever-expanding literature on the subject should be warmly welcomed In his preface, the author indeed asserts that the *Encyclopedia* will "provide an easily accessible treatment of important medical topics which will engage the general reader while providing students of both history and medicine with a fund of information complementary to their specialties"

As one would expect in an encyclopedic work, the book is alphabetically arranged. Among the entries are a number dealing with the history of medicine The essay listed under "Medical History" contains a brief historiographical review of the subject, and there are separate articles on medicine in ancient Egypt, Mesopotamia, China, India, and the Greco-Roman world, as well as on Hebrew and medieval medicine The health professions are represented by entries for the medical profession, nursing, barber-surgeons, and pharmacy Most basic sciences, such as anatomy, physiology, pathology, and immunology, receive individual treatment The same can be said of clinical specialties such as gynecology, midwifery, neurology, ophthalmology, pediatrics, psychiatry, radiology and surgery

On illness, the *Encyclopedia* contains twenty-six entries for some of the most important infectious diseases, such as plague, cholera, leprosy, tuberculosis, syphilis, smallpox, and yellow fever A number of other ailments are also featured, for example, cancer, coronary heart disease, diabetes, rheumatism and scurvy. In addition, there are broader groupings such as allergy, deficiency diseases, and mental illness.

Medical therapeutics are discussed in brief essays on chemotherapy, inoculation, sulfonamides, antibiotics, and vitamins, among others The author has written special articles on acupuncture, anesthesia, bloodletting, drug abuse, and hypnosis, as well as on "irregular medicine" (homeopathy, hydropathy, osteopathy and chiropractic), Mesmerism, and quackery.

As with any effort of synthesis, one is bound to find inconsistencies and imbalances which reflect to some extent the individual biases and preferences of the reader. In this instance, McGrew devotes only four pages of text to Greco-Roman healing (Hippocratic-Galenic medicine), barely a page more than the space given to medicine in ancient India. Islamic contributions, in turn, are buried in a single page on medieval medicine If material is categorized by traditional periods, why not have entries on the medicine of the Renaissance and later centuries? I found a nice article on the Brownian system, based in part on my own writings, but why were there no articles on other important medical systems? Modern obstetrics is unfortunately lost among articles on gynecology, the forceps, and midwifery Why devote five articles to deficiency diseases and only one to mental illness?

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In spite of such shortcomings, however, the book will be useful, especially for non-specialists and general readers. The additional reading suggestions at the end of each entry are quite helpful, although here again, presumably because of the time publication takes, the author's references are now somewhat dated The double-column text and typeface employed are easy on the eyes and the price of the book is moderate by present standards.

Reviewed by GUENTER B. RISSE, Department of the History and Philosophy of Health Sciences, University of California at San Francisco, California.

DIANE R. KARP et al (comp). ***Ars Medica: Art, Medicine, and the Human Condition*** (catalogue of an exhibition at the Philadelphia Museum of Art). Philadelphia. University of Pennsylvania Press for the Philadelphia Museum of Art, 1985. xiv + 231 pp Ill. $42.50.

Sixteenth-century readers probably would not have understood the need to justify the relationship between art and medicine. Modern technology has separated the two, so that today we find it curious that doctors and painters once belonged to the same guild, that of their mutual patron saint, Luke. Early readers, however, would have known without thinking twice that doctors and painters derived the tools of their trades—healing elixirs on one hand and colors on the other—by the same means—pulverizing, mixing and distilling raw ingredients Today the Catholic Church no longer dominates medicine, and physicians as a rule have little time for artistic pursuits. Yet *Ars Medica* makes it clear that art and medicine continue their intimate connection even in this modern age.

This book marks the fourth in a series of Philadelphia exhibitions geared toward illustrating the link between art and medicine There have been previous attempts to do so from both sides. However, medical writers sometimes lack art historical awareness—mis-identifying artists, misreading their intentions, or failing to recognize common artistic conventions Art historians can be equally insensitive to the history of science, inventing inappropriate medical interpretations, or, more commonly, failing to recognize a medical context altogether. Happily, *Ars Medica* is as sophisticated medically and historically as the illustrations are fine and beautiful. These qualities are achieved by the sensitive and knowledgeable organization of material into four major sections dealing with the human condition: "Anatomy," "Healers," "Disease, Disability and Madness," and "The Cycle of Life."

Some of the visual choices for the section on "Anatomy" were intended for medical audiences, such as the elegant woodcut from Vesalius's *Fabrica* (1543) in which a skeleton leans gracefully on a crutch while eloquently addressing us with a rhetorical gesture. Others are pure artistic invention, like Rauschenberg's *Booster* (1967) which features another skeleton—this time seen through the dispassionate eye of a modern X-ray machine. Interestingly, Vesalius's "medical" image emerges as the more traditionally "beautiful" of the two, though it was not created specifically for an artistic audience Thoughtful juxtapositions like this one occur frequently in the book, causing us to pause before using words like "artistic" and "medical" to describe art.

Similar intellectual creativity distinguishes the other three divisions of the book, where images created centuries apart are paired in order to illustrate their common roots in the human condition. The second part, for example, presents "Healers" in all guises, from the earliest medieval visions of Christ as spiritual physician to scathing satires of all-too-human quack doctors The third section, on "Disease, Disability and Madness," presents the dark side of the human condition—from the privileged intellectual genius of Durer's *Melencolia I* to the degraded depths of modern alienation seen in Richard Bosman's *Suicide, 1980–81* The final division of the book, devoted to "The Cycle of Life," begins with the birth experience and ends with an examination of the ways in which society perceives death. Here, Hans Sebald Beham's *Young Woman Accompanied by Death as a Jester* (1541) compares ironically with George Tooker's *The Mirror* (1975) in which a modern girl admires her reflection while ever-present death peers over her shoulder.

The intelligence of this book is demonstrated not only by the inspired choice of images and their intriguing organization, but also by the thoughtful and informed catalogue entries written by noted experts in the histories of art and medicine. The helpful glossary of artistic terms and the list of selected readings are also welcome to readers in both fields. These qualities distinguish *Ars Medica* as a landmark in the interdisciplinary study of art and medicine. As such, it is surely the happiest conjunction of fine and healing arts yet to appear in print.

Reviewed by LAURINDA S. DIXON, Department of Fine Arts, Syracuse University, Syracuse, New York.

JOHN CULE and TERRY TURNER (eds) ***Child Care through the Centuries*** (Papers given at the Tenth British Congress on the History of Medicine, Swansea, Wales, 6–8 April 1984, and other invited contributions). Cardiff, Wales STS Publishing for the BSHM, 1986 v + 223 pp £10 60 (paperbound) (Available from STS Publishing, Welsh School of Pharmacy, UWIST, Cardiff, Wales, U.K.)

The title is somewhat misleading for the essays in this book address the history of pediatrics rather than the more general issue of child care. Nor is this a comprehensive history of pediatrics but a collection of papers on various topics with disparity in the degree of specificity and of originality, and in style However, such is almost unavoidable in a publication of congress papers and the historian can only be grateful for this addition to the sparse number of texts on the history of pediatrics.

Among the more general papers, Jean Cleary describes the nineteenth-century emergence of pediatric hospitals in spite of the eighteenth-century view that small children should not be separated from their families, and the more recent return to earlier opinion Cleary is particularly explicit on the causes of and delays in the modern reform movement, but only dwells lightly on the reasons why pediatric hospitals were established in the first place No mention is made, for example, of medical opinion that such hospitals were needed to advance pediatric knowledge John D Atwell addresses the history of pediatric surgery while acknowledging that justice cannot be done to such a complex topic in the time allowed His paper serves as a guide to the human, technical and institutional factors that need to be considered for a fuller history Two papers, Sydney Brandon's on the early history of psychiatric care of children and Sheila J. Wallace's on the evolution of developmental medicine, illustrate the problems in searching for historical equivalents to modern concepts of neurophysiological, intellectual, emotional and behavioral development In earlier centuries, innate wickedness might have been the counterpart of more recent perceptions of maladjustment, and the ungovernable child seen as a challenge to theology rather than to medicine. Mental deficiency was more obviously definable as disease, and so its earlier interpretation forms the greater part of Brandon's essay These two papers are to some extent complemented by Peter H Thomas's account of cerebrospinal disorders in children as described by an early nineteenth-century physician, and by John Cule's description of a sixteenth-century Welsh text on infant care as well as child management and education.

In a paper on child abuse, Bernard Knight demonstrates that in 1860 Ambroise Tardieu, professor of legal medicine at Paris, wrote a long article whose contents have been forgotten but which anticipated much current knowledge on battered children. Also intriguing is Richard A. Collacott's well documented account of tetanus as a major killer of newborn babies in the Outer Hebrides during the nineteenth century Attention to very simple sanitary measures completely eliminated this scourge, which had previously killed from one quarter to two thirds of the newborns on the island of St. Kilda. Concerning a common but usually non-lethal disease, infantile rickets, Valerie A. Fildes provides a comprehensive summary of medical advice and treatment in pre-industrial England

Two papers on Arabic contributions to the development of pediatrics and one on the evolution of infant nutrition as a science in the Netherlands are welcome illustrations that pediatrics has always been a trans-national enterprise Most stimulating and provocative is G. A. Russell's thesis that Ibn Tufayl (Abubacer, c 1105–85) significantly contributed to later European enlightenment thinking about the acquisition of knowledge through experience. Abubacer's Arabic text, describing the mental development, through sensory experience and association of ideas, of a baby growing up among animals on an island deserted by humans, was translated into Latin in 1671 at Oxford and entitled *Philosophus Autodidactus* The book was widely circulated among scholars, and, furthermore, the translator, Edward Pocock (1648–1727), was a pupil of John Locke, who in 1671 drafted his *Essay Concerning Human Understanding.* Russell advocates further inquiry to establish the extent of Abubacer's influence.

On a practical matter, the binding of this book is pathetic. My review copy fell apart early on in the proceedings but the remains will be nurtured as a specimen of what, since the early 1930s, has become a very rare species—a pediatric anthology

Reviewed by ELIZABETH LOMAX, Medical History Division, Department of Anatomy, University of California, Los Angeles, California

JEAN-CLAUDE DOUSSET. ***Histoire des médicaments· des origines à nos jours.*** Paris. Payot, 1985. 405 pp. Ill F150,00 (paperbound)

Perhaps the most obvious gap in the historiography of medicine is the lack of an adequate general history of the drugs upon which physicians, as well as non-physicians, have always relied as a major tool for restoring health. An "adequate" history would, at the very least, include clear descriptions of the *evidence* which permitted each remedy to enter medical usage, to remain there, or to be abandoned. That is, the story of each drug should meld both its supposed and actual pharmacological properties with the physiological and pathological concepts held by its users, and also include some discussion of the drug's efficacy, whether provable or only assumed. The most common story line, that "Drug X was used in the treatment of

Condition Y," is not very helpful, and can often be bewildering, as, for instance, when X is a cathartic and Y is now known to be a viral infection.

Unfortunately Dousset's book does not fill the gap. It is another in the long line of volumes, like Wootton's *Chronicles of Pharmacy* (1910), which are chiefly lists. Each chapter is devoted to what Dousset regards as a "rhythmic oscillation" in his story of the inexorably uni-directional progress of medicine: the ancient civilizations, the Middle Ages, the Renaissance, the seventeenth century (which he labels "le siècle de la raison," although his examples appear to contradict the principles of reason), and each of the next three centuries. He does outline the major intellectual accomplishments in each period, but he then inserts discussion of the drugs selected without tying them specifically to relevant contemporary medical or scientific concepts. He also neglects many continuities that have persisted over the centuries. Although Dousset extols the pharmacological triumphs of the years since World War II as his historical zenith, he glides over the historical experimentation behind even modern drugs; he does describe their pharmacological properties clearly, but that is not history.

If a "Whig version" of the evolution of drug usage is possible, Dousset has written it, chiefly by avoiding questions of evidence for drug efficacy as well as information about what drugs actually *do* to the human body. He probably sets his stage most succinctly when he avers that "Les acquis thérapeutiques de la période hippocratique présentent des caractères positifs indéniables" (p. 44). Had he adopted Chauncey Leake's concept of "protopharmacology" (the study of drugs before the first academic pharmacology laboratory was established in 1860), his synthesis and conclusions might have been different. Although Leake's pioneering *Historical Account of Pharmacology to the Twentieth Century* (1975) can be accused of some of the conceptual errors evident in Dousset's book, Leake did summarize and assess much of the evidence which permitted the introduction of new drugs into medical usage.

The story of Withering's clear demonstration of the clinical efficacy of digitalis is noticeably absent from Dousset's list-book (but the isolation of digitaline by French chemists is mentioned). This omission probably has two causes. One is that Withering's work was an innovative study of true efficacy, whereas Dousset seems to assume that most historical drugs were truly efficacious without exploring the pharmacological, much less the non-pharmacological, reasons for their acceptance as effective remedies. The other probable cause of the omission is that this is an almost entirely Francocentric, and perhaps xenophobic, work. This same factor may have precluded any mention of the roots of modern pharmacology in Bucheim's laboratory and its Germanic flowering in the nineteenth century. Although Dousset's overall emphasis seems to be on pharmacy rather than pharmacology, his obvious familiarity with the latter only underscores this omission.

No detailed references are provided, all but one of the mere twenty-one items cited in the bibliography are French publications, and that one must have been cited because Dousset took two figures from this reviewer's article in that journal (without attribution). Only a few factual errors are detectable. Some of the information contained in the text is potentially useful, but it is already lost because there is no index. The book closes with a number of rather eccentrically chosen and arranged appendices that bear no relationship to the text. It will be relatively easy for non-specialists in French to read, but even the large *Petit Larousse Illustré* does not provide several necessary definitions. Dousset's intended audience is a bit puzzling; it is probably the interested non-medical reader His book will not help historians of medicine writing in English, chiefly because it includes no original scholarship or even useful citations

Reviewed by J. WORTH ESTES, Department of Pharmacology and Experimental Therapeutics, Boston University School of Medicine, Boston, Massachusetts

BRIAN M DU TOIT and ISMAIL H ABDALLA (eds) ***African Healing Strategies.*** Owerri-New York-London· Trado-Medic Books, Conch Magazine, 1985 ix + 279 pp. Ill. $39.95 (cloth); $20.00 (paperbound).

This book reprints thirteen papers presented at a conference held at the African Studies Center of the University of Florida in March 1984 Two articles by Du Toit which appeared in journals in 1971 and 1980 have been added Like most such volumes, this is something of a potpourri of essays of varying length and importance The fifteen contributions are grouped into three clusters dealing with Islamic, Christian, and scientific influences on African therapeutic ideas and practices Many of the contributors are anthropologists, which perhaps explains why there is much more material on religious and psychological aspects of therapy than on actual medicines or surgical practices

Islam has had an enormous cultural impact on much of Africa in the last millennium or so, but surprisingly little attention has been paid to the influences of Islamic medicine on indigenous African medical systems. Two essays in this book, those by Abdalla on the medical writings of the leaders of the jihad which swept northern Nigeria in the early nineteenth century, and by Robert Stock on the rather limited role of religious teachers in healing among the modern inhabitants of the area, are particularly interesting. The Hausa and other West and North African peoples draw upon the later, religion-dominated Islamic medicinal tradition, rather than the more scientific classical writers such as Ibn Sina or Al-Razi.

The second section consists of Du Toit's two papers on healing among the urban Zulu of South Africa, a general overview of healing and religious syncretism, and a useful case study of health care systems in Zambia. Carolyn Sargent presents a fascinating account of changing childbirth practices among the Bariba of northern Benin The most significant piece in this section, indeed in the whole book, is John Janzen's "Changing Concepts of African Therapeutics· An Historical Perspective" Janzen stresses the pragmatic approaches of Africans, both as healers and as patients. He, like many other contributors, shows that Africa has long been receptive to new medical ideas and that the notion that "traditional" African medical systems were rigid and unchanging is totally false. Janzen argues that studies of African therapeutics are incomplete without considering the historical dimension. And, like religion, medical beliefs and practices are often spread over large regions, not restricted to one cultural or linguistic group

The third group of five papers contains discussions of the current relations between "traditional" and modern practitioners in Zimbabwe by Ann Beck, and an acerbic but rather unsatisfying essay on the decline of Tswana medicine in the face of missionary pressure. The last essay, a very unfortunate melange of bad history and over-reverence for appropriate technology, argues that many African countries would be better off without antibiotics or vaccines. Surely one can recognize the supreme importance of better nutrition and safe water supplies without rejecting penicillin and smallpox vaccine!

This volume contains no major surprises and is only a modest contribution to the rich and diverse medical history of Africa Important topics like the use of patent medicines and the widespread illicit use of modern pharmaceuticals receive little attention. The popularity of injections, seized upon by quacks and honest healers alike, has obvious implications for the spread of hepatitis and AIDS However, readers of the *Bulletin* with an interest in the continent would profit from sampling some of the essays, especially those of Janzen and Sargent The book is not very well produced Typographical errors abound and table 3 is missing from page 46.

Reviewed by K. DAVID PATTERSON, Department of History, University of North Carolina at Charlotte, Charlotte, North Carolina.

JOSEPH NEEDHAM with the collaboration of LU GWEI-DJEN *Science and Civilisation in China*, vol 6, ***Biology and Biological Technology, Part I: Botany.*** Cambridge and New York: Cambridge University Press, 1986 xxxii + 718 pp. Ill. $95.00.

PAUL U. UNSCHULD. ***Medicine in China: A History of Pharmaceutics.*** Berkeley-Los Angeles-London: University of California Press, 1986. xii + 366 pp Ill $65.00

The study of traditional Chinese botanical literature is a poorly developed field, whether in terms of the history of science or of the history of Chinese medicine, though botany was very much in the service of medicine in traditional China. Thus the virtually simultaneous appearance of two major works on the topic is a welcome event. It is doubly welcome since the two volumes in question—part one of the botany volume of Joseph Needham's *Science and Civilisation in China,* and the second volume of Paul Unschuld's *Medicine in China,* devoted to the botanical literature of Chinese medicine—represent such diverse approaches.

The two approaches are respectively comparative and Sinological. The Needham volume seeks to study the Chinese past by examining it from the point of view of worldwide technological and scientific development, minimizing the specifically Chinese cultural context of its subject The Unschuld volume, on the other hand, looks at its topic exhaustively in a Chinese context in exclusively Chinese terms.

Both approaches are legitimate and well established, and each has specific advantages and disadvantages. The comparative approach is particularly useful in allowing us to evaluate Chinese topics immediately, in their larger context Its primary disadvantage, on the other hand, well illustrated by the Needham volume, is that the comparative method has often resulted in ethnocentric approaches to Chinese developments from the standpoint of purely Western cultural assumptions, taken as the pattern against which all is to be evaluated. A purely Sinological approach, on the other hand, not only evaluates traditional China from a Chinese point of view but also offers a methodologically highly reliable product—especially in the hands of a Paul Unschuld—which can, if necessary, be used as a firm basis for comparative purposes by others (the reverse is rarely the case). Pure Sinology, however, can also result in pedantic, obscure scholarship with meaning only to the few. With these thoughts in mind, let us now look in detail at the two works

The Needham volume, part of section 38 of the planned complete work, is in five parts· an introduction, a section on plant geography, one on "botanical linguistics," a discussion of Chinese botanical literature and a section on "plants and insects in man's service." Still to come are sections (by Georges Metailie) on traditional botanical treatises, plant description and illustration, Chinese knowledge of the life of plants, horticulture and the "influence of Chinese flora and botany on modern plant science." The Unschuld volume consists of a general introduction, a section on the Shen-nung herbal tradition, a lengthy discussion of the various branches of herbal literature and a final section on modern Chinese pharmacopeia from the point of view of the traditional literature Both volumes are well illustrated Both have full bibliographies and notes (very complete, as always, in the Needham volume).

The Unschuld volume is a much expanded version of a 1973 work which appeared only in German and had a very limited printing (*Pen-ts'ao 2000 Jahre traditionelle pharmazeutische Literature Chinas* [Munich. Heinz Moos, 1973]). Much of the new material is in the form of (additional and expanded) translations of introductions and sample selections of texts discussed These translations are one strong point of the book, which offers many hitherto largely unknown texts in a readily accessible form. They make the volume important as a source book as well as a reference work Other strong points are its comprehensiveness, the reliability of its information, the author's good grasp of his subject in all its many aspects and the well chosen illustrations. Its major defect, growing out of its approach, is the tendency for important points to become lost in a maze of detail. Another problem is the sometimes inadequate annotation of translated material (which is frequently difficult going in any case)

However, these defects are minor in comparison to those of the Needham volume Its problems include the age of the text (it was completed in the early 1970s and takes little notice of the wealth of material published since then, the earlier Unschuld volume, for one example) and, most serious, a badly flawed approach. Needham and his team have insisted too rigidly upon placing Chinese traditional botanical literature in a Western context and of evaluating its achievements almost solely in Western terms

For example, do we really need detailed comparisons between Li Shih-chen's work and that of Linnaeus before we can appreciate Li's achievements (comparisons with Linnaeus and other Western botanists are pursued throughout the text, for Li Shih-chen particularly, pp. 308ff) Likewise, is it not a distortion of the entire Chinese medical botany tradition to call the *pen-ts'ao kang-mu* and other representatives of the Chinese *pen-ts'ao* tradition "pandects of Natural History" (see the discussion on pp 220ff) In fact, of all the Needham volumes seen to date by this reviewer, the present one seems the most artificial in forcing its subject into a Western framework, even to the point of seriously decreasing the value of the abundance of useful evidence that it does present

In conclusion, both the Needham and Unschuld volumes have much to recommend them. The latter in fact, despite minor defects, is destined to become a classic It will be the standard reference work in the field for the foreseeable future The Needham volume, however, which will be widely used because of the prestige of the *Science and Civilisation in China* series, suffers from its serious conceptual flaws. It is useful, and, when read with its flaws in mind, is a gold mine of information about the history of Western as well as Chinese botany Unfortunately, however, it continues to propagate the basic misconceptions about the nature of Chinese "scientific" thought that have characterized the Needham effort—as valuable and as instructive as it has been to us all—from the very beginning.

Reviewed by PAUL D BUELL, Center for East Asian Studies, Western Washington University, Bellingham, Washington

PAUL U UNSCHULD (trans) ***Nan-ching. The Classic of Difficult Issues.*** With commentaries by Chinese and Japanese authors from the third through the twentieth century. Berkeley-Los Angeles-London University of California Press, 1986 (The Chinese Medical Classics). viii + 760 pp. $69.75

Nan-ching· The Classic of Difficult Issues is a translation of an important Chinese medical work, the *Nan-ching* (written around the first century A.D) The translation is the third book by Professor Unschuld to appear in a series entitled "Medicine in China", the two earlier volumes, also published by the University of California Press, are *Medicine in China A History of Ideas* (1985) and *Medicine in China. A History of Pharmaceutics* (1986).

Unschuld's scholarly translation constitutes a major contribution to the study of Chinese medicine Physiological theories, diagnostic methods, and related therapies (principally acupuncture) had attained a high degree of sophistication by the first century A.D. when the *Nan-ching* was composed. As Unschuld observes, "the *Nan-ching* covers—in an unusually systematic fashion—all aspects of theoretical and practical health care perceivable within the confines of the yinyang and Five Phases doctrines, as defined by the original medicine of systematic correspondence" (p 4) Along with the slightly older *Huang-ti nei-ching,* the *Nan-ching* played a seminal role in the later development of Chinese medicine In order to show the traditional view of the book, Unschuld has also translated large portions of the pre-modern commentaries to the *Nan-ching.* Other valuable additions to the translation are the prolegomena which include a detailed discussion of the history of the book and its place in Chinese medicine, appendices listing Chinese and Japanese annotated editions of the *Nan-ching,* and a brief bibliography of contemporary Chinese scholarship on the *Nan-ching.*

Unschuld's translation appears at an opportune time in scholarship on traditional Chinese medicine. The fact that traditional medicine is still viable in contemporary China and is drawing the attention of non-Chinese as an alternative to Western medicine is an impediment to the critical evaluation of the history of Chinese medicine At a time when pre-modern Western medicine has become history, many scholars conducting research on Chinese medicine, both in China and elsewhere, regard the current practice of traditional Chinese medicine and the justification of its theories in terms of modern medical science as their main objectives The assumption that a pre-modern theoretical model can be given a new foundation in modern science is problematic, as witness the continuing debate over the nature of *ch'i* and the medical value of acupuncture; and the issue of its contemporary status tends to obscure the significance of the Chinese system of medicine as it was practiced in earlier centuries In this respect Unschuld's translation is a boon to historical study The reader will find that Unschuld has rendered the *Nan-ching* in a way that maintains the "pre-modern" conceptual framework. Read together with the translated commentaries, one comes to appreciate the ideas and attitudes that informed traditional Chinese medicine.

Unschuld's method of translation is to present a very literal reading of the Chinese (the *Nan-ching* itself is printed in Chinese along with the translation). Given the difficult language of a highly technical and theoretical work like the *Nan-ching*, Unschuld deserves praise for the quality of his translation. One could wish, however, that more information had been incorporated into the book—in the form of illustrations, tables, glossaries defining key terms, or supplementary explanations—as a convenience to those who are not already versed in Chinese medicine. There is a glossary of technical terms, but it is arranged by Chinese characters so that only those who know some Chinese can effectively consult it. Further, while the glossary identifies occurrences of the terms in the *Nan-ching*, Unschuld's own notes to the text provide little explanation of what the terms mean Probably the best strategy for reading the translation of the *Nan-ching* is to keep a copy of *Medicine in China. A History of Ideas* by one's side, in which Unschuld discusses basic concepts and explains his translations of certain key terms (e.g., "vapors," "finest matter influences," or "influences" for *ch'i*). These cavils aside, let me reiterate that *Nan-ching· The Classic of Difficult Issues* is a significant book, one that sets a standard for scholarship on the history of Chinese medicine

Reviewed by DONALD HARPER, Department of Oriental Languages, University of California, Berkeley, California

PIERRE J. PAYER. ***Sex and the Penitentials: The Development of a Sexual Code, 550–1150.*** Toronto-Buffalo-London: University of Toronto Press, 1984. xi + 219 pp $24 95

Penitentials were practical manuals used by priests during the early Middle Ages that listed specific penances for different types of sins. Pierre Payer's study describes the sexual contents of these handbooks, and traces the reception of penitential literature by the Church during the early period of the formation of canon law.

A central argument of *Sex and the Penitentials* is that the specific sexual material contained in these manuals reflects actual behavior. If there are canons against lesbian sex using an artificial phallus, for example, this means that the practice existed to a certain extent during the European Middle Ages. The author refers more than once to earlier historical treatment of the penitentials which claimed that references to the more exotic sexual acts had little to do with reality, or to translators of the texts who consciously omitted the sexual canons. Payer characterizes this approach as "overly modest" (p. 119), and comments that if the penitentials had not reflected the reality of human behavior, they would have had little use in the society.

Sex and the Penitentials gives us a guided tour of the listed offenses by grouping them into categories such as "heterosexual life of the married," "masturbation," and "bestiality," but although the study presents considerable detail, there is very little analysis. The author states, for example, that a man involved in a passive homosexual act merits the same penance as an adulterous woman or a sodomist, but he provides no sense of what this might mean in terms of societal values Payer discusses *laws*, not *ideas;* the book is long on facts and short on interpretation.

In the few instances where the author does allow himself to look at the larger picture, we find some interesting perspectives. In the last page-and-a-half of the conclusion, he points out that although we find a negative, legalistic and authoritarian approach to sexual behavior in these manuals, the penitentials were not the cause of this repressive attitude. Early medieval society itself was at fault: it developed virtually no theological reflection on this central aspect of human life, and so penitentials, laws, rules and regulations were the only writings that existed on sexuality. Indeed, Payer comments further, that despite the sameness and monotony of these writings he "had a feeling while reading these manuals that they were engaged in strenuous combat against urges and forces in human nature which were long in being brought to heel. The newly converted peoples of western Europe seem to have had a strong attachment to a more diversified, open, and freely expressed sexuality than could be countenanced by the Christian ethic" (p. 121). Interpretive statements such as these could have provided a framework for his study, instead of being buried in its last two or three paragraphs.

Sex and the Penitentials instead concentrates on listing offenses and penances (e.g., texts which hold that an animal involved in bestiality with humans should be killed), describing ninth-century condemnation of the penitentials because they did not contain references to conciliar and papal authority, and tracing the rehabilitation of the penitentials during the tenth through twelfth centuries as some of their texts were incorporated into canonical collections. It contains numerous guides to the texts themselves, five appendices, and lengthy footnotes and bibliography. Its value is largely as a reference work; an interpretive study on this subject is yet to be done.

Reviewed by HELEN RODNITE LEMAY, Department of History, SUNY at Stony Brook, Stony Brook, New York.

ARNALDI DI VILLANOVA. ***Opera medica omnia, XVI: Translatio libri Galieni de rigore et tremore et iectigatione et spasmo.*** Edited, with English commentary, by MICHAEL R. MCVAUGH. Barcelona: University of Barcelona, 1981 (Seminarium historiae medicae granatense; Universidad de Granada). 111 pp. $20.00 (paperbound).

ARNALDI DI VILLANOVA. ***Opera medica omnia, XV: Commentum supra tractatum Galieni de malicia complexionis diverse.*** Edited, with Spanish commentary, by LUIS GARCÍA BALLESTER and EUSTAQUIO SÁNCHEZ SALOR; ***Doctrina Galieni de Interioribus.*** Edited, with English commentary, by RICHARD J. DURLING. Barcelona: University of Barcelona, 1985 (Seminarium historiae medicae cantabricense, Universidad de Santander). 393 pp. $30.00 (paperbound).

These volumes are the second and third to appear in the series of Arnald of Villanova's *Opera medica omnia.* The first, volume II, *Aphorismi de gradibus,* appeared in 1975. Eight further volumes are promised, but the series when complete may contain as many as nineteen volumes. Such a lengthy enterprise is justified by Arnald's significant role in the development of scholastic medicine at Montpellier at the turn of the fourteenth century. Galen had survived piecemeal into the early Middle Ages in the West, and the first wave of Salernitan and Spanish translations from the Arabic in the eleventh and twelfth centuries did little to establish a proper Galenic corpus. Only in Arnald's time did accurate and full translations of Galenic texts really become available for teaching and commentary purposes. Arnald was both a translator and a commentator, helping to put the "new Galen" alongside the "new Aristotle."

Michael McVaugh's *Translatio libri Galieni de rigore* provides the reader with an introduction (28 pages); a note on the text and existing manuscripts (twenty are listed); the Latin translation itself (46 pages, with critical notes), and indices of names, terms used in the text, and of manuscripts quoted. The introduction alone is a classic account of the different fortunes of a Galenic text in the hands of translators from late antiquity to the fourteenth century. From a medical history point of view, McVaugh shows clearly the way in which the four concepts of *tromos* (tremor), *palmos* (palpitation), *rigos* (rigor), and *spasmos* (spasm, taking three forms—*tetanos, emprosthotonos,* and *opisthotonos*) fared at the hands of Latin medical authors. Unfortunately no Arabic text comparable to that translated by Arnald has ever been found, but comparison with the Greek original shows the faithfulness of Arnald's work. Curiously, Arnald's translation did not serve as a basis for further commentary or for use in the medical curriculum. It remains as a monument to the most philosophically minded of the Montpellier teachers.

Volume XV contains two works: the Arnaldian commentary on Galen's treatise *De malicia complexionis diverse,* and the compendium on pathology drawn from Galen, probably by Arnald, *De interioribus.* The much longer commentary has been edited from two surviving manuscripts, and the Galenic text from a further two. The editors' *Introducción* is in Spanish, and its 103 pages are well worth tackling even for those less familiar with the language. Here too the "new Galen" comes in for discussion, but so do the linguistic and literary issues raised by the text; most important for the medical historian, the editors explain the scholastic devices which determine the commentary form.

The most important theoretical issue addressed in Arnald's commentary is the definition of *mala complexio:* the pathological complexion, or illness, whether in the body as a whole or in one of its members, which arises when there is a change in the relative equilibrium of the qualities (hot, cold, wet, and dry). Each member has its own equilibrium, appropriate to its function. Once illness has destroyed the functioning of a member, a new state of equilibrium is reached—a non-natural equilibrium rather than a natural. But illness is a process in which the natural and the non-natural complexions are in unresolved conflict, and Arnald calls this *mala complexio diversa.* From the point of view of the practicing physician, he is confronted with this *mala complexio diversa* in his patients, and accordingly Arnald goes on to show how it is manifested in different fevers.

The second text in this volume is drawn from Galen's chief work on pathology, *De locis affectis,* books I and II only. The work is found in three manuscripts, and is significant mainly because it shows how closely the author—probably Arnald—stuck to Galen. The volume concludes with the same apparatus of indices as supplied by McVaugh. The editorial standards set by both volumes are extremely high (though I have been unable to check transcriptions), and no serious student of medieval medicine can afford to be without them.

Reviewed by PETER MURRAY JONES, King's College, University of Cambridge, Cambridge, England, U.K.

LUIS S. GRANJEL *Historia política de la medicina española* Salamanca, Spain. Universidad de Salamanca, 1985. 233 pp. No price given (paperbound)

The title of this book is a misnomer: it is not a political history, but rather a survey of the history of medicine in Spain divided into very broad periods from antiquity to the 1930s. The narrative chapters (the first six) are so concise that they provide hardly more than a skeletal framework They are followed by two chapters on the place of physicians in Spanish society and culture since the Renaissance The next five chapters treat specific areas of medical knowledge (morphology, pathology, surgery, medical specialties, and pharmacology), and three concluding chapters deal with public health

The most interesting passages are those relating to the eighteenth century and the relationship between secularization and medical ethics Granjel argues that, at that time, the medical profession ceased to be ruled by religiously-inspired norms, and these were replaced by secularized administrative precepts Once a professional ethic replaced a religious one, physicians then embarked upon a struggle to increase their wealth and prestige as a class. (Note that Michel Foucault's construction of the transition to a modern medical structure is just the opposite. In his view, the French Revolution's ideal of a rationalized medicine was brought about by organizing physicians like the clergy and giving them powers over bodies similar to those exercised by the clergy over souls)

Although the book is a useful compendium of a generation's research on Spanish medical history, no historians are cited and a "bibliographical guide" lists only nine works, all by Granjel himself

Reviewed by THOMAS F. GLICK, Department of History, Boston University, Boston, Massachusetts

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FRANTZ TARDO-DINO *Le Collier de servitude: La condition sanitaire des esclaves aux Antilles Françaises du XVII^e^ au XIX^e^ siècle.* Paris. Editions Caribéennes, Agence de Coopération Culturelle et Technique, 1985 306 pp F70,00 (paperbound).

The subtitle of Tardo-Dino's book points to his principal interest, the health of the black population in the French Antilles from the inception of slavery in the early seventeenth century until its final abolition in 1848 This is more than a compendium of tropical diseases and their treatment by contemporary medical practitioners, however. It is an effort to link the high mortality and morbidity rates experienced by eight hundred thousand blacks with their total environment, from their capture and sale in Africa to their hard labor in the slave gangs of the sugar and coffee plantations of the French islands of Saint-Domingue, Martinique, and Guadeloupe.

Tardo-Dino divides his study into three sections: The first briefly treats the slave trade on the African coast, the unbelievable physical conditions of the Atlantic crossing by slave ship, and the superficial "refreshment" of the "captives" with needed nourishment, exercise, and bathing, in addition to palm-oil to enhance their marketability on the islands The second is a systematic description of all aspects of the slaves' material existence—plantation management, work rhythms, housing, clothing, food, punishments—as well as aspects of their social behavior—the slave hierarchy, runaways, sexuality, and some attention to family and religion. The third and largest section catalogues the various types of diseases and sicknesses among whites, blacks, and mulattoes and their prescribed treatments, both European and West Indian, and ends with a twenty-five-page lexicon of topical medical plants. Here the author's medical professionalism is most apparent. Far from endorsing the indiscriminate use of herbalist remedies, Tardo-Dino warns the reader, as if his history doubled as a guide to public health on the islands in 1985: "One cannot repeat enough that the doctor must remain sole judge of the appropriateness (*bien-fondé*) of a medical prescription" (p. 241)

Although the author's consideration of the daily material existence of the slave is thorough, indeed exhaustive, he does not weigh these environmental factors or establish any precise linkages between a specific condition such as a calcium deficient diet and a particular disease such as rickets, or the refusal to distribute shoes to slaves for reasons of social control and the prevalence of foot ulcers and other skin diseases. Instead, Tardo-Dino relies essentially on the cumulative effect of all factors from diet and personal hygiene to labor conditions and medical ignorance to explain high mortality and morbidity rates and very low birth rates over two centuries Little attention is given to historical evolution, except regarding planter policies toward natality and types of slave punishments, so that there is little sense of deteriorating material and psychological conditions, which undoubtedly took place in the last third of the eighteenth century among both slaves and freedmen

A greater emphasis on development would have thrown light on the changing strategic role of a relatively healthy "class" of creole *gens de couleur* and *affranchis* who were partly immune to the great killers—yellow fever and malaria—and to other diseases as well. Many of the mulattoes lived and labored in the highlands, where they grew, husked, and sorted coffee beans, rather than in the lowlands where the black slave gangs planted shoots, cut cane, and boiled sugar, day and night for six months of the year. Different work routines surely made a difference. Moreover, the very low birth rate among black slave women deserves more discussion. What of infanticide? What is the medical opinion about long lactation periods? What about psychological tensions, surely not without medical ramification, born of a deeply racist society? Not that a conclusive explanation is possible given the evidence available, but more speculation about the attitudes and mentality of whites and blacks that lie behind behavior would be welcome

Understandably, the author shows a certain partiality to the medical profession even as it performed on the islands in the seventeenth and eighteenth centuries. First, he is impressed by the medical manuals (the bibliography is very useful in this respect) indicating a serious interest by Europeans in tropical disease. Second, he does not reduce all white surgeons—on shipboard, in town or on the plantation—to ignorant adventurers who could find no medical positions in Europe Not all of their remedies followed the prescribed European medical practice of bleeding and evacuation There were a few successes: inoculation for smallpox had immunized perhaps 50 percent of the black population by 1790, and those dismal plantation infirmaries probably deserve credit for reducing lockjaw as a major cause of high infant mortality. On the whole, however, Tardo-Dino evokes a deplorable picture of public health on the three French islands, not very different from that described by Richard B. Sheridan (*Doctors and Slaves. A Medical and Demographic History of*

Slavery in the British West Indies, 1680–1834, Cambridge, 1985) for the British West Indies

Reviewed by ROBERT FORSTER, Department of History, The Johns Hopkins University, Baltimore, Maryland

UTE FREVERT *Krankheit als politisches Problem, 1770–1880: Soziale Unterschichten in Preussen zwischen medizinischer Polizei und staatlicher Sozialversicherung.* Göttingen, West Germany: Vandenhoeck & Ruprecht, 1984 (Kritische Studien zur Geschichtswissenschaft, no 62) 469 pp. DM92,00 (paperbound).

For some reason the social history of medicine has not yet drawn the interest of German historians in the way it has French, British, or American ones. Ute Frevert's *Krankheit als politisches Problem,* therefore, is especially welcome, as she works within a far broader conceptual framework than that usually envisioned. Although it is true that her book still concentrates on a rather traditional geographic area and chronological period—Prussia in the nineteenth century—she succeeds in breaking new ground and thus her book is a valuable addition to the very sparse collection of literature we presently possess on the social history of medicine in central Europe. One can only applaud the author's endeavor to expand the scope of the history of medicine, yet one cannot escape the fact that the result is mixed A reader has the distinct feeling that there are really two books hiding between the covers—one a very adroit, subtle examination of mutual benefit or self-help societies and the beginnings of medical insurance systems, the other a far less convincing discussion of the "politicization of health" and the "medicalization of poverty" from 1770 to 1848

In the first half of her work, Frevert reviews enlightened health propaganda and medical professionalization as elements in a wider policy of social control or social discipline She argues that the "process of politicizing health" had as its goal "the generalization of health-conscious standards of behavior and medical care in all spheres [of life] and at all levels of the social system" (p 45). According to this line of reasoning, the state and physicians collaborated in a move (her argument tempts one to write conspired!) to pare away at the variety of therapies and healers that characterized the pre-modern world and to reduce a rich medical pluralism to a single medical omneity, ruled by physicians whose judgments would reach into most, if not all, phases of life. Frevert recognizes, quite rightly, that in the eighteenth century such a program remained at best incompletely realized To support her contentions, she offers an excellent summary of the voluminous contemporary literature churned out on populationism, medical police, and popular medical enlightenment. Unfortunately, she rarely goes beyond prescriptive literature or policy statements to peer more closely at what physicians, or, more important, what state organs, such as privy councils or boards of health, did on a day-to-day basis In short, she seems to acknowledge little contrast between rhetoric and reality, equating all too facilely the first with the second. Most of this results, I believe, from an insufficient (indeed, practically non-existent) exploitation of archival materials for this early period One would have liked, for instance, to see some proof of the sad economic plight of physicians other than their own testimony Furthermore, a closer look at archival sources, or even at secondary material, would have saved her from the error of believing in the unquestioning compliance of pastors with physicians or with the state in a process of medicalization (see John Stroup, "Protestant churchmen in the German Enlightenment: Mere tools of temporal government?" *Lessing Yearbook,* 1977, *10.* 149–89). She accepts the theory of medicalization uncritically, instead of rigorously testing its validity.

These flaws virtually disappear in the second and much stronger half of her book Her research appears impeccable, solidly based on archival sources and less reliant on prescriptive literature She traces the often convoluted history of medical insurance plans (the *Krankenkassen*), delineating in impressive detail the reasons for their formation, their later development, and their changing relationship with the state. She skillfully leads us from the problems of dislocated guildsmen in the Vormärz to the rather different issues, situations, and concerns of factory workers after 1848. She develops a far more balanced and complete view of the *Krankenkassen* than previously available, viewing them both from "above," the perspective of government and employers, and from "below," the perspective of workers. Here Frevert is at her best, deftly untangling the conflicting motives of factory owners, the Prussian state, local government, and workers She skillfully lays out the shifting issues involved in medical insurance, describing the tensions between liberal ideas of personal freedom and the interests of government that characterized the debate on social policy in the period after 1848 until the passing of the 1883 law which provided medical insurance for a large percentage of wage earners Her ability to discriminate between rhetoric and reality is clearly evident here and allows her to assess and apply such loaded concepts as discipline and control more cautiously, while ignoring the temptation to employ simple explanations of economic self-interest. Here, too, she refuses to be seduced by myths, boldly challenging accepted wisdom that, for example, the workers were politically educated for agitation through the *Krankenkassen* or that industrialists pigheadedly resisted all forms of medical insurance to which they had to contribute. This skepticism is strikingly different from her easy acceptance in the first part of a stereotypical image of physicians as greedy graspers for power, who ruthlessly shoved aside all other competition

In sum, Frevert has produced a book with more strengths than weaknesses Perhaps she attempted too much, certainly this reviewer feels she would have been well-advised to jettison the burdensome theory of part one which produced a programmatic statement rather than a historical evaluation Finally, although one searches in vain for a consistent argument or even a single theme that succeeds in gluing the two parts of the book together (physicians, for example, are major actors in the first part, but tend to disappear from the story later on), it is packed with information The second half is a reliable and well-crafted treatment of an important part of medical and social policy history in the nineteenth century

Reviewed by **MARY LINDEMANN**, Department of History, Carnegie Mellon University, Pittsburgh, Pennsylvania

GIUSEPPE BARONIO *On Grafting in Animals* [Degli Innesti Animali] Translated by JOAN BOND SAX, with an introduction by ROBERT M GOLDWYN Boston. Boston Medical Library in the Francis A Countway Library of Medicine, 1985. 87 pp. Ill. $125.00 (limited edition).

Giuseppe Baronio (1758–1810) is an individual lost in history because his work was published in a small edition in Milan (1804) and in an uncommon language—an archaic Latinized Italian idiom. Now, due to the efforts of Robert M Goldwyn, Joan Bond Sax, and the Boston Medical Library, the contributions of this great investigator have been resurrected. His work deals with skin grafting, a subject now accepted and well understood In Baronio's time, this was not the case. Using carefully designed experiments, he observed the acceptance or rejection of skin grafts His book, the result of these studies, is considered the first great work in plastic surgery after Gaspare Tagliacozzi It is certainly the earliest recorded effort at successful skin grafting in animals.

The book starts with a thorough and scholarly introduction by Goldwyn on Baronio's background and contributions. This section gives important insights on Baronio's writings and his somewhat idiosyncratic behavior. In addition to skin grafting, other topics he explored were organ regeneration, wound healing, and electrical phenomena. He was also interested in prison reform and humane treatment of inmates Goldwyn notes that Baronio's monograph was the result of twenty years of effort and investigation and was the first scientific attempt at autogenous skin transplantation in animals with planned study and accurate experimental observations

The work is in seven parts. After an introductory statement, Baronio gives an account of the historical aspects of Tagliacozzi's work, and of the earlier nose grafting by the Maratha surgeons of India and the Brancas of Sicily The third section deals with the grafting of teeth in man by John Hunter and the fourth is a review of John Hunter's work on grafting in the cock The fifth chapter I particularly enjoyed as it deals with the methods of wound healing and skin grafting of contemporary charlatans. The sixth section, though, contains the basis of this work. Here we see in original and methodical detail the author's efforts at documentation of acceptance and rejection of skin grafts. He records the progress of the skin graft in each case and also experiments with different graft thicknesses Baronio experimented with the differences resulting from delays in placing the graft and observed that the longer one waited, the worse the "take" In the seventh section, Baronio discusses debridement and its use in "freshing up" the graft site and the improved graft that results

The volume is published in a limited edition of 325 copies by the Bird and Bull Press of Newtown, Pennsylvania. The publication represents another excellent effort by this press. It is a tastefully-designed volume guaranteed to look elegant on its owner's bookcase The book comes highly recommended for its appearance, the thoughtful and scholarly introduction by Robert Goldwyn, and most important, its re-introduction of a great Italian scientist's contribution to the field of plastic surgery and grafting techniques

Reviewed by JAMES TAIT GOODRICH, Departments of Neurological Surgery and Pediatrics, Albert Einstein College of Medicine/Montefiore Medical Center, Bronx, New York.

JOHN HARLEY WARNER. ***The Therapeutic Perspective: Medical Practice, Knowledge, and Identity in America, 1820–1885.*** Cambridge, Massachusetts, and London: Harvard University Press, 1986 x + 367 pp. $32.50.

Traditionally historians have paid less attention to the history of medical practice and more to the evolution of theory The reasons are not difficult to understand Published writings are readily available and lend themselves to historical analysis. Medical manuscript records, on the other hand, are less abundant; scholars also face formidable interpretive problems because of the idiosyncratic entries of physicians. The result is a curiously disjointed historiography of medicine that deals less than adequately with the precise relationships (often assumed rather than demonstrated) between theory and practice.

In recent years some American historians have attempted to remedy this defect by analyzing medical practice and its links with theory. Several essays by Charles E. Rosenberg suggested a future direction. John Harley Warner's study of nineteenth-century American medicine is another harbinger of this new approach. Much, of course, is known about nineteenth-century medicine Yet our knowledge of medical *practice* is somewhat limited. It is clear, for example, that the use of drugs changed sharply. By the end of the century the view that interventionist measures should grow out of the findings of experimental science replaced the previous faith in therapeutic empiricism. The process by which this occurred, as well as the implications for practitioners, however, has been generally glossed over, thus giving rise to a rather simplified portrait of a century during which American medicine underwent fundamental changes

By using hospital and other unpublished medical records as well as traditional medical writings, Warner has produced an important analysis that will contribute to the current re-evaluation of practice and theory (and especially their relationship) Nineteenth-century American physicians, he notes, derived their professional identity from their interventionist skills rather than from any theoretical expertise. Judgment based on experience, not laboratory knowledge, defined the successful physician. Faced by challenges to their professional legitimacy, they began to reject the rationalistic systems associated with such figures as William Cullen and Benjamin Rush after 1820 in favor of therapeutic empiricism. In so doing they developed a new epistemology based on the principle of specificity. Medical therapeutics had to be related to the specific characteristics of the individual as well as to such environmental characteristics as geography and climate. Local knowledge, not universally binding laws, governed the practice of medicine, and the goal of physicians was to restore the natural balance

The new synthesis, according to Warner, lasted but briefly. By the 1850s the "normal" began to replace the "natural" as the paradigm of health Through careful examination of the records of the Massachusetts General Hospital and the Commercial Hospital of Cincinnati, he traces the decline of heroic therapies and explores the cognitive implications of change In essence, therapeutic knowledge began to be based on a commitment to universalism rather than specificity, there was a lessened interest in the unique characteristics of both the patient and the environment. The new therapeutic epistemology also led to a novel sense of professional identity that based therapeutics on experimental physiology; knowledge rather than behavior was the characteristic that defined physicians The transition was neither easy nor rapid;

to overthrow the experiences of the past posed the danger of calling into disrepute the entire profession In a series of illuminating chapters Warner traces with sensitivity the debate among physicians in the 1860s and 1870s over the foundations of their profession The changes that were taking place had important implications for physicians and their patients, as well as medical education and institutions. Oddly enough, the decline of the principle of specificity meant that the role of the social and physical environment in the etiology of disease was relegated to a position of secondary importance That the therapeutic claims of those committed to a universalist epistemology were exaggerated was hardly noticed; the stage was set for the dramatic changes that transformed the practice of medicine as well as health care institutions.

What is unique about Warner's contribution is the strategy he has employed in delineating the characteristics of nineteenth-century medicine First, he describes in great detail the scientific and intellectual presuppositions of medical thought, thus demonstrating the value of epistemological analysis Second, he deals with the transition from specificity to universality in unusually perceptive terms, and its implications for the status of physicians and the practice of medicine. Third, he evaluates the medical debates that accompanied change while avoiding the temptation of imputing superiority to any of the competing views. Fourth, he delineates the manner in which American physicians reinterpreted European medical thought. Finally, and perhaps most significant, he deals with actual medical practice, which both qualifies and sheds light upon discussions of theory.

In reading this important work I had only two criticisms. The first is substantive. It is clear that there was a major shift in both medical epistemology and practice between the 1820s and 1880s Warner describes these changes and provides considerable illustrative empirical data. What is lacking, however, is a persuasive explanation that accounts for change. Toward the close he argues that the rise of experimental laboratory science played a significant part in therapeutic reorientation Although undoubtedly true, this explanation is incomplete. Therapeutic change by Warner's own data began somewhat before the rise of modern laboratory science. More important, Warner neglects developments external to medicine that may have played a significant role. The absence of a larger social and intellectual context, in other words, detracts from an otherwise fascinating internal analysis, and also illustrates some of the dangers of treating the history of medicine in a cultural and intellectual vacuum.

My second criticism pertains to style Warner too often writes in a dense and complex manner that obfuscates as much as it illuminates Thus he creates needless barriers even to the informed reader Indeed, his weakness in accounting for change may reflect, at least in part, the character of his prose as well as the quality of his analysis These shortcomings, however, should not obscure my overall judgment that Warner has produced a significant work that will prove of enduring value.

Reviewed by GERALD N. GROB, Institute for Health, Health Care Policy, and Aging Research, and Department of History, Rutgers University, New Brunswick, New Jersey.

JOHN RICHARDSON ***Arctic Ordeal: The Journal of John Richardson, Surgeon-Naturalist with Franklin, 1820–1822.*** Edited by C STUART HOUSTON Kingston and Montreal· McGill-Queen's University Press, 1984. xxxiii + 349 pp. Ill. $Can 29.95.

This handsome volume is a welcome addition to the literature of polar exploration It should also prove of interest to students of nineteenth-century medicine Here for the first time is published the journal of that "old polar bear," Sir John Richardson, which he kept during Lieutenant John Franklin's first overland expedition to the Arctic Ocean of 1819–22 This venture was part of a series of British attempts to find the Northwest Passage in the peaceful years 1818–45, following the defeat of Napoleon. It was a journey mainly by boat across northern Canada to the shores of the polar sea, which complemented a sea-borne expedition led by Parry westward through Lancaster Sound. Its main geographical achievements were to confirm the discoveries of Samuel Hearne in the late eighteenth century and to chart a considerable portion of the north coast of America eastward from the mouth of the Coppermine River Its scientific achievements owed much to John Richardson, one of several surgeon-naturalists, who contributed to the advancement of geology, botany and zoology during the nineteenth century

A modern biography of Richardson by the physiologist R. E. Johnson appeared a decade ago At about the same time, C. Stuart Houston, the editor of this volume, also edited the journal of Midshipman Robert Hood, another member of this first overland expedition of Franklin. The appearance of the present volume means that the journals of three out of four of the naval officers' journals have now been published. This volume consists of some 350 pages of text, of which about half is Richardson's journal, dating from late August 1820 to mid-December 1821. An editorial introduction, commentary, and a number of scientific appendices make up the remainder of the book. Of these, pages 186 to 197 deal with the medical and nutritional aspects of the expedition, which very nearly ended in disaster from starvation during the return journey across the Barren Lands Four of the five Englishmen lived, but only two of the eleven Canadian voyageurs Houston considers the reasons for this discrepancy.

An exhibition held in Lincolnshire last year to celebrate the bicentenary of Franklin's birth in 1786 was called "The Man Who Ate His Boots." One is reminded of this on p 196, where Houston observes "Perhaps one of the most interesting new understandings gained from Richardson's journal concerns the nutritive value of moccasins and caribou-skin blankets. Franklin speaks of eating 'roasted leather' and Richardson gives a little more detail . 'Every scrap of shoe leather is now highly prized and after boiling is greedily devoured.' "

The editor has the advantage of knowing the Northwest Territories very well and the illustrations by H. A. Hochbaum were done recently in the footsteps of Franklin's party There is much detail in the maps, which have not been reproduced so successfully.

To end this review, here is a quotation from Richardson's journal for 23 September 1821, which illustrates not only the plight of the overland expedition, but his own plain, yet descriptive style (p 138).

> Mr Back and the Ind Interpreters Pierez and Adam were sent on before, to hunt and await our arrival at the river Soon after we started, the men found some pieces of the skin and a few bones of deer that had been devoured by the wolves in the spring They halted and

> lighted a fire, and rendering the bones friable by burning, devoured them with avidity They also ate several of their old shoes Thaw in the afternoon Walked until dark, fording in the course of the day two arms of the lake The canoe was broken today and left behind, notwithstanding every remonstrance The men had become desperate and were perfectly regardless of the commands of their officers

Reviewed by ANN SAVOURS, National Maritime Museum, Greenwich, London, England, U K

ELAINE SHOWALTER *The Female Malady: Women, Madness, and English Culture, 1830–1980.* New York Pantheon Books, 1985 xii + 312 pp Ill. $19.95.

Elaine Showalter directs her attention to a wide range of topics: the actual social history of modern women, the cultural stereotyping of femininity, the rise of the psychiatric profession, of psychiatric theory and practice, the fate of mentally ill women and the therapies to which they have been subjected, the literary representation of mad women, both by male and female novelists and by women sufferers, and so forth Her contention is that all of these histories are fundamentally interconnected, and, in particular, that such interconnections need collectively to be grasped before we can understand what it has been to be a "mad woman," to suffer from "the female malady." Or, put another way, if we made the mistake of isolating the "mad woman" from these contextual and textual dimensions, we would end up with the misconception which is psychiatry, or psychoanalysis itself.

Every era has produced its own variations upon these common themes The early Victorian age highlighted the family ideal, domesticated therapy, and feminized insanity (madness was an overabundance of sensibility). Small wonder then that it produced a frame of values in which women who failed to conform to the domestic criteria were commonly diagnosed as mad, while those who did—in reality and in fiction—experienced such norms as maddening In its turn, the "psychiatric Darwinism" of the late Victorian period endorsed biologistic conceptions of sex roles, and stigmatized the "new woman" who sought to break free as "hysterical" (a straitjacket loosed only by the paradoxical emergence of shell shock as "male hysteria" in the First World War)

Hereabouts, Freudian psychoanalytic therapy promised to emancipate women from old biologism, but in reality it merely fettered them within more insidious "mind forg'd manacles," not least the male analyst as therapist/rapist (both Freud and R. D. Laing are deftly deconstructed) The process repeats itself Showalter concludes by pointing the way forward· "Until women break them for themselves, the chains that connect madness with the feminine, like Blake's 'mind forg'd manacles,' will simply forge themselves anew" (p 250).

A summary as terse as this can hardly begin to hint at the rich texture, subtle interpretations, synthetic skill, and, not least, the humanity which animate this superb book Showalter's literary insights repeatedly match her grasp of the modulating historical pulse The less plausible feminist readings (e g, hysteria as heroic rebellion) are put firmly in their place, and insofar as this text constitutes a tract against the "wrongs of woman," it proceeds largely by leaving the facts to speak for themselves Of course, Showalter's interpretation is contentious and open to criticism. For instance, I feel deeply unhappy about the label "psychiatric Darwinism," just as it seems to me profoundly questionable whether Freudian theory ever opened up brighter prospects for female psychiatric release. But the glory of this book lies in how often its judgment is acute, and in how triumphantly literary analysis, social history and the history of psychiatry are integrated to illuminate each other.

Reviewed by ROY PORTER, Wellcome Institute for the History of Medicine, London, England, U K.

PAT JALLAND and JOHN HOOPER (eds) ***Women from Birth to Death: The Female Life Cycle in Britain, 1830–1914.*** Atlantic Highlands, New Jersey: Humanities Press International, 1986. xii + 337 pp $35.00.

This is a well-organized and original collection of documents drawing primarily, but by no means exclusively, on medical sources It picks up the kind of general issues we might anticipate in a volume on the female life cycle, for instance, childbirth and birth control, but within and in addition to these, there are some imaginative surprises On pages 73–74, we find at last some mention of the history of the sanitary towel (from the *Transactions of the Obstetrical Society of London* for the 1880s and 1890s) and a lively exchange from the *Lancet* of the 1840s about the wisdom of allowing husbands to be present at childbirth (pp. 159–61).

The editors counsel their readers wisely, warning that the often prescriptive literature represented in the volume should not necessarily be equated with Victorian behavior; certainly this point has been argued strongly in respect to Dr Acton's well-known strictures about the absence of female sex drive, although it is perhaps more important to understand why so many women acquiesced in this view of their sexuality, at least in public The first part of the collection reminds us of the enormous weight of Victorian medical opinion expounding the idea that biology is destiny and that the uterus controls the mind. Medical ideas about female sexuality were but one element in a complete theory of sexual difference that was used throughout the late nineteenth and early twentieth centuries to justify female subordination, and against which, as Jalland and Hooper point out, women were largely powerless Denied access to medical and scientific training, their replies were bound to lack authority.

Ideally, the introductions provided by the editors should have helped the reader a little more. Because the documents chosen include material written by female as well as male physicians, for example, it is important to know a bit more about the writers: how it is that the two heavily-relied-upon women doctors, Chesser and Scharlieb, appear to share so many of the views of their male colleagues and the identity of the mysterious physician A. L Galabin, who both addressed the Obstetrical Society on the sanitary towel and included what appears to be a fairly extensive discussion of criminal abortion in his 1900 *Manual of Midwifery* (pp 144–67). In some cases, there have been confrontations between historians on particular issues, for example, the effects of tight lacing, to which it would have been useful to refer readers

On the whole, the choice of documents is more successful when representing a closely defined issue. Childbirth and birth control are well worked areas and the range of material in this volume is narrow. But it is extremely interesting to have, for example, some letters from women about menopause, the diary comments of parents on the deaths of their children and of husbands on the deaths of their wives in childbirth, and the medical debates over chloroform and clitoridectomy. The latter of course resulted in the expulsion of clitoridectomy's chief proponent, Mr Baker Brown, from the Obstetrical Society, because of his failure to obtain the consent of patients and their husbands to the operation and because of the underlying concern about the practice as a form of quackery and as such, a threat to the profession, a significant comment on Victorian medicine's view of the female patient

Reviewed by JANE LEWIS, Department of Social Science and Administration, London School of Economics and Political Science, London, England, U.K

G. G. MEYNELL ***The Two Sydenham Societies: A History and Bibliography of the Medical Classics Published by the Sydenham Society and the New Sydenham Society (1844–1911).*** Folkestone, England· Winterdown Books, 1985. viii + 192 pp. £13.00 (paperbound).

Founded in 1844 and 1858 respectively, the Sydenham Society and the New Sydenham Society were British publishing associations. As such they collected annual membership fees and used them as capital for a variety of publishing projects. This method of subscription publishing was not unique to them but had been pioneered by British historical societies earlier in the century. The innovation of the Sydenham Society was to introduce this method to medical publishing. Unfortunately, the Sydenham Society was short-lived because it was poorly organized and chaotically run. But the New Sydenham Society survived—and even thrived at times—because of the energy and organizational skills of its secretary, Jonathan Hutchinson (1828–1913).

Between 1844 and 1911 the two societies published about 175 medical works in the following categories. a) editions and translations of works by important historical figures, e.g , Greenhill's and Latham's Sydenham, Robert Willis's Harvey, Adam's Hippocrates, and his Aretaeus; b) translations of contemporary medical literature from the Continent, e g , Smith's translation of Schwann, Whitley's of Diday, and Murchison's of Frerichs, c) original works in English, e.g., Hutchinson et al , *Atlas of Skin Diseases* and the well-known *Lexicon of Medicine and the Allied Sciences* by Power et al.; d) digests and surveys of contemporary medical literature, e g , *Year-book of Medicine* and *Medical Digest;* and e) medical history, e g , the translation of Hecker's *The Epidemics of the Middle Ages* and *Annals of Influenza 1510–1837*

This detailed, complex, but lucidly arranged book is an important contribution to the history of medical publishing. Meynell not only documents the achievements of the two Sydenham Societies but also provides a wealth of information about their publications that will be valuable to anyone who reads nineteenth-century medical literature in English. Besides its value as a publishing history, this monograph is an important source for the study of the transmission of European medicine to North America. Many of the works first published by the two societies were published again in the United States, some in the well-known and widely sold Library of Standard Medical Authors of William Wood & Co Moreover, North American physicians and practitioners, who (presumably) could not afford the cost or time of travelling to study in European laboratories and medical schools, learned the latest in European medicine by subscribing to the Sydenham societies Indeed, so many American physicians subscribed to the New Sydenham Society that it suffered a crisis in 1862 when many of them cancelled their memberships at the onset of the Civil War (p. 165). In short, *The Two Sydenham Societies* will be of interest to a wide variety of historians of nineteenth-century British and North American medicine.

Reviewed by **PHILIP M. TEIGEN**, History of Medicine Division, National Library of Medicine, Bethesda, Maryland.

ROBERT M. YOUNG. ***Darwin's Metaphor: Nature's Place in Victorian Culture.*** Cambridge and New York: Cambridge University Press, 1985. xvii + 341 pp. $44.50 (cloth); $15.95 (paperbound).

This collection of six essays deals with the nineteenth-century debate on "man's place in nature" and especially analyzes the role of the Darwinian theory of evolution and its controversial reception in this debate. First published in about 1970, these essays have not only a common subject matter but also a common historiographical and methodological perspective. They look upon this nineteenth-century debate not as *scientific,* in the sense of esoteric and specialized discussion fragmented into academic disciplines, but as a broad movement in which scientific, philosophical and ideological components are hardly separable and which influenced deeply the cultural and political life of its time as well as, on the other hand, being influenced by this cultural and political life.

This perspective guides Young's approach to the origin and development of the Darwinian theory itself and he, therefore, is very critical of the dominant "internalistic" restrictions of historiographical research: "The Darwin industry has reverted to the norms of scientists and scholars rebuilding the very boundaries which the nineteenth-century debate showed were not arbitrary but were in the service of ideological forces which place the role of the professional expert at the service of culture (as ornament) rather than aligning expertise with forces for democracy and progressive social change" (pp. xi–xii). He reproaches the "internalistic" Darwin industry not only for being ideological, but for missing the very character of its subject matter: it isolates Darwin first from the mainstream of contemporary naturalism, of which it was in fact a part, and second it demarcates his evolutionary theory sharply from those of his predecessors and contemporaries (p. 79).

The range of Young's "contextual" approach is demonstrated especially in the title essay, which, by a detailed analysis of the crucial role that the analogy between "artificial" and "natural" selection played in the development, exposition, and reception of Darwin's theory, makes plausible the idea that not only were there philosophical, theological, and ideological debates *about* this theory but that they played a constitutive role *in* it.

Reviewed by **KURT BAYERTZ**, Center for Science Studies, University of Bielefeld, Bielefeld, West Germany.

HARRY W. PAUL ***From Knowledge to Power: The Rise of the Science Empire in France, 1860–1939.*** Cambridge and New York: Cambridge University Press, 1985 415 pp. $49 50

The latest book by Harry Paul on French science deals with a wide range of topics, including biology, the place of science in agriculture, the importance of Catholic universities, the system of scientific publication, and the new schemes for financing science in the first decades of the twentieth century. In most cases it presents a mass of new information extracted from an enormous variety of published and unpublished sources As far as I know, Harry Paul has no equal in this regard. However—and this is perhaps the other side of the coin—his way of arguing is sometimes difficult to follow, and the reader may sometimes find himself lost in a maze of more or less unprocessed information. Of course it is important for the historian never to sacrifice the density of reality for the pleasure of displaying a superb albeit too simple reconstruction, to strike the right balance between general trends and the irreducibility of particular individuals Yet I would have liked to find in the book rather more explicit conclusions or working hypotheses. As we all know, risking strong interpretations is part of our job.

The first chapter—the great scientific mutation at work in France throughout the Second Empire and the Third Republic—is an excellent synthesis dealing with the history of science faculties, the *Ecole Pratique des Hautes Etudes,* the Republic of Professors, and so on The following two chapters handle biology, but differ greatly from each other. The first of them is in the nature of a pamphlet which tries to prove that an emphatic "no" is the right answer to the question of whether Auguste Comte and his group had any significant influence on French biology. The way the problem is posed is perhaps not the one I would have chosen but the subtle and acerbic tone of this chapter was enjoyable. The next chapter, on the contrary, is far more neutral and descriptive and puts an interesting and new stress on the laboratories of marine biology

The most strongly focused and probably the most clearly conceived chapter in the book is the one which considers the industrial connection of university science Though it overlaps some works already published either by Paul himself or by others such as Robert Fox, Mary Jo Nye or Terry Shinn, it would be a mistake not to study it closely On the other hand, the next to last chapter—the reader will understand that I cannot discuss everything here—is somewhat puzzling Dealing with the funding of science, it asks very relevant questions but without giving an impression of what is decisive or what is worth remembering. Here the problem raised earlier is seen at its most extreme

One last remark If the subtitle of the book, "The rise of the science empire in France," were to be taken literally, Paul could be criticized for certain omissions—above all, a chapter on military research It is somewhat meaningless to reproach the author for this, however, as it is unfair to expect one person to do everything others have not yet done. Besides, this reviewer appreciates all too well how difficult it is to gain access to military archives, how French authorities of any kind regard vast amounts of documentation as "delicate," and how blurred and uncertain are the boundaries of the power which allow them to keep to themselves whole domains of the collective history of their country.

Reviewed by **DOMINIQUE PESTRE**, Study Team for CERN History, CNRS/CERN, Geneva, Switzerland.

HENRI H. MOLLARET and JACQUELINE BROSSOLLET. ***Alexandre Yersin, le vainqueur de la peste.*** Paris: Fayard, 1985 (Les inconnus de l'histoire) 320 pp. Ill. F95,00 (paperbound)

A well documented, exhaustive work about Alexandre Yersin (1863–1943) has been lacking. We know the importance of the plague in the history of humanity; Yersin's name has been associated with the discovery of its bacillus Yet we are ignorant about the rest of his life and work In this book, Henri Mollaret and Jacqueline Brossollet update forgotten works and retrace the history of this pioneer of bacteriology. The task was difficult because Yersin never asked for honors, and he was a secretive man But historians had one useful available source, a set of letters, a thousand letters written by Yersin to his mother between 1884 and 1905, and to his sister from 1906 to 1932. This source does set up a potential trap for the researcher, that is, the temptation to concentrate on psychological aspects of a man's life and anecdotes. We know what can result from such an approach: a romantic history that enhances the genius of a man and the adventures in his life without ever trying to figure out his social environment. I point this out because Henri H. Mollaret and Jacqueline Brossollet have chosen the other approach and avoided the trap. Yersin has demystified the plague, they have demystified Yersin.

To prove my point, Henri Jacotot and Noel Bernard have said, time and time again, that Yersin met Emile Roux and Pasteur because he had injured himself while doing an autopsy on a man killed by rabies. This is false. Yersin's letters provide no evidence for such a legend The meeting was more casual It was Rémy, head of Didier Richet's clinic who, upon request, took Yersin to the Rue d'Ulm where Pasteur was trying vaccines against rabies. There, Yersin soon became indispensable. To offer a second example, there has been continuing insistence on the variety of activities of Yersin and also on his extravagances Again this is misleading Yersin raised animals in order to have the necessary specimens for the Pasteur Institute at Nha Trang in Indochina. If he organized a plantation, it was to feed these animals, if he became a meteorologist, it was to plan his crops better

This book does more than explore myths about Yersin. The first researches of Yersin on experimental tuberculosis or "Yersin type," and the first, fundamental, works on diphtheria together with Roux are discussed Mollaret and Brossollet clearly establish the priority of Yersin over Kitasato in the discovery of the plague bacillus. They show how Yersin had seen the clinical and anatomical identity of human and rat plagues and how he had initiated the epidemiological study of the illness. Also Paul Louis Simond is not forgotten. two dense pages are devoted to his brilliant discovery of the role of the flea in the transmission of plague.

But the main contribution of the book is, without any doubt, the reconstruction of the scientific and cultural milieu of the time in which Yersin lived. First of all, we get for France and the creation of the Pasteur Institute precious information about medical studies, and, above all, about the teaching of bacteriology at the end of the nineteenth century. For Indochina, we learn about French penetration. This includes not only information about the foundation of the medical school of Hanoi and its economic woes (tropical agronomy, zootechny) but also Mollaret and Brossollet offer a sensible account of the political, medical and economic reasons that justified the organization of the colonial branch of the Pasteur Institute in Nha Trang.

The book has three aims· to put an end to stubborn legends, to clear up some points of the history of medicine, and to restore the cultural and scientific context of an extraordinary adventure The project was ambitious It is a success. One can appreciate the quality of the information and the precision of the analysis. Many will read with pleasure this study that gives life to a vaguely forgotten adventurer of bacteriology· Alexandre Yersin

Reviewed by FRANÇOIS DELAPORTE, Instituto Investigaciones Historicas, Universidad National Autonoma de México, Mexico City.

VICTORIA A. HARDEN. ***Inventing the NIH: Federal Biomedical Research Policy, 1887–1937.*** Baltimore and London· The Johns Hopkins University Press, 1986 xiii + 274 pp. Ill. $32.50.

In 1976 our nation was buried in an avalanche of self-praise as we celebrated (forever it seems) our bicentennial, now we seem destined to experience similar excesses as we anticipate the bicentennial of the Constitution And, as the nation assembles for depiction in yet another Norman Rockwell painting of national pride, we should not forget that 1987 will witness an important birthday, this one the centennial of the National Institutes of Health (NIH), which we shall celebrate with no less intemperateness Already planned for the centennial, for example, are a grand ball, numerous regional symposia, a public television media event in four parts which will consider landmarks in biomedical research, and, to be sure, public speeches delivered by a brigade of extraordinary medical scientists and Nobel laureates, representatives of the Executive agencies and the authorizing and appropriations committees of Congress, and by ordinary citizens—all joining together in loud chorus to wish the NIH, that "brilliant jewel in the crown of H.E.W.," as Wilbur Cohen called it (p. 179), a Happy One Hundredth Birthday.

What better place to start the celebration than with a reading of Victoria Harden's splendid history of the antecedent events leading in the 1930s to the "invention" of the NIH, which she examines "as a part of the larger culture and federalization of all sciences in the twentieth century" (p. 4)? Harden's account, vividly set forth and meticulously researched, tells the story of the intermingling of science, public health, and politics as the nation made up its mind as to how it wished to organize, administer, fund, and eventually institutionalize its federal health and scientific enterprises. It is a story of ideas and men, of the elegance and ambiguity of compromise, of petty ambitions, jealousies and frustration, of persistence, and of historical accident. It is a story worth telling and reading, centennial celebration or not.

Harden's book is divided into four parts. It starts with the origin of the NIH, traced to its beginnings in a one-room bacteriological laboratory established in the Marine Hospital on Staten Island, New York, in 1887. Here, with scientific apparatus modeled after those used by Robert Koch, Joseph Kinyoun fulfilled his broad responsibilities —investigating the waters of New York Bay, determining the health of merchant seamen and immigrants; preparing cultures, sera, and biologicals; supervising quarantine and disinfections; preparing exhibits, and teaching the elements of bacteriology to Marine Hospital Service officers in 1894

Woven into the story of the Hygienic Laboratory are the political events surrounding the campaign for the first National Board of Health and the attempt to legitimize the Marine Hospital Service as the federal government's public health bureau The campaign was successfully consummated in 1902 when legislation was enacted enlarging the Hygienic Laboratory, redesignating the head administrator as Surgeon General, and renaming the agency the Public Health and Marine Hospital Service How the Service and its laboratory grew is told expertly, with finesse and patience, in this book. We discover how and why it expanded its mandate to include pathology, zoology, nutrition, chemistry, scientific pharmacology, and also the study of diseases with broad social and economic implications, such as hookworm, pellagra, and those associated with polluted water or dangerous trades. We also find out how the larger mandate led to additional major political campaigns, before and after World War I, when Congress directed its attention to both the Service and its Hygienic Laboratory

What makes Harden's book so successful is that the reader moves effortlessly from the scientific to the political arena, and back again We learn not only of the "baneful" impact of the Budget Director, Herbert Lord, and the mischievous Smoot-Mapes bill, which would have brought the ponderous Veterans Bureau into the Service, thereby engulfing the Service's education and public health efforts, but also the ploys and counterploys of federal officials, such as the Surgeon General himself, whom one would have thought would eschew politics by virtue of their positions, and the development of the antecedents of the two major bills, which moved over the years in parallel and in unison, but separate and distinct from one another; one designed to strengthen the Service and the other designed to establish and fund a scientific entity dedicated to both applied *and* basic research.

In the discussion of these bills one finds James Tobey and his organizational history of the federal health bureaucracy, and also an analysis of his reorganization efforts, and other important figures, such as Charles Herty, Francis Garvan, and Julius Stieglitz who outlined the need for first, a private sector, and then, when this proved impractical, a federal, basic research institute We meet also Congressman James Parker and Senator Joseph Ransdell, sponsors of the major bills in question; learn of the testimony of public health, insurance, medical, and clinical brahmins called before Congressional committees; review behind-the-scenes lobbying efforts; learn also about the difference a President, a Congressman, or a Surgeon General, makes in the design, in the furtherance or forestalling, and in the implementation, of public policy.

When reading the text I could not help but wonder about the role of Senator Ransdell, without whose efforts there would have been no NIH. His bill was signed into law by President Herbert Hoover in 1930, and when the bill won Senate concurrence, he rose to express his satisfaction: "I am the author of this bill which I believe is fraught with incalculable good to humanity; it is my dream come true" (p. 158) And yet Ransdell's name has long been forgotten. What he did was no less significant than the well documented deeds of his Congressional successors· Hill, Fogarty, Magnuson, Yarborough, Kennedy, and Rogers; but whereas these names are writ large in our memories and intimately associated with the rise to prominence of our national science enterprise, Ransdell's name, in Harden's words, has "slipped into near oblivion" (p. 165). It is good to have him resurrected.

When one considers the NIH's present mandate and achievements (set forth in Harden's epilogue), the attention the NIH receives in the media, our national expectations, the budget battles between the Office of Management and Budget, Congress and the special interests, and even the specific criticism—that the NIH has had a baleful and malign effect on medical school education, disrupted the integrity of our universities, contributed to overspecialization, and directed money and talent away from what some consider are the nation's real health problems—one thinks back to Kinyoun and Rosenau, Herty and Garvan, Cumming and Parran, Parker and Ransdell, and cannot help but conclude that the outstanding and useful history that Harden has presented to us is really an account of those who "builded better than they knew."

Reviewed by ARTHUR J VISELTEAR, Section of the History of Medicine, Yale University School of Medicine, New Haven, Connecticut

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S E D SHORTT *Victorian Lunacy: Richard M. Bucke and the Practice of Late Nineteenth-Century Psychiatry.* Cambridge and New York Cambridge University Press, 1986 xvi + 207 pp Ill $29 95.

The Anglo-American asylum superintendents of the late nineteenth century might well be considered a "lost generation" in the history of psychiatry. Overshadowed by the therapeutic optimism of their predecessors and the dynamic psychiatry of their successors, the late nineteenth-century specialty seems distinguished chiefly by its failures: the return to a pessimistic somaticism, the rise of large custodial institutions, and the loss of professional prestige and autonomy

S. E D. Shortt's beautifully written biography of the Canadian alienist Richard Maurice Bucke is all the more welcome because it challenges the conventional view of the late nineteenth-century asylum as an intellectual wasteland As head of the London Asylum in Ontario from 1876 to 1902, Bucke cut an influential, if slightly eccentric, figure in both the American and English specialties. Despite the institutional constraints on his career—and Shortt does not make light of these—Bucke nonetheless managed to sustain a lively and wide-ranging intellectual life. Using his career and writings as a focal point, the author presents a valuable and perceptive overview of the late nineteenth-century intellectual terrain

Throughout his career, Bucke struggled, as did many medical thinkers of his day, to develop a "secular physiology of the mind," freed from metaphysical speculation and grounded in empirical observation His two chief medical works, *Man's Moral Nature* (1879) and the better known *Cosmic Consciousness* (1901), represent a "microcosm of Victorian biomedical thought" (p 63) In these works, Bucke combined positivism, associationist psychology, evolutionary theory, and physiology to expound a theory linking the evolution of man's moral nature to the development of the sympathetic nervous system Since the moral sense was a recently acquired trait, he argued, it was all the more likely to lapse, thus insanity was a form of moral "atavism," a reversion to a more primitive stage in moral development.

In Bucke's work, as in that of William James and others of the period, devotion to empiricism coexisted with a keen interest in spiritual phenomena Having himself had a profound mystical experience as a young man, Bucke rejected strictly materialist views of human nature, his belief in a transcendent cosmic consciousness (which formed the basis for his long friendship with the poet Walt Whitman) paralleled the late nineteenth-century "Boston School" in laying the foundation for a new appreciation of the unconscious in early twentieth-century psychiatry

While primarily an intellectual biography, this account of Bucke's career constantly strives to interpret his ideas within the context of his professional and institutional experiences. For example, Shortt ties the popularity of degeneration theory both to generalized fears of the "dangerous classes" and to the "pauperization" of the nineteenth-century insane asylum; then suggests that the growing interest in the unconscious represented a "widening of the psychiatric gaze" to include a more attractive middle class clientele (p. 162) Shortt also includes a fascinating discussion of Bucke's short-lived experiments with gynecological surgery in the 1890s, which he interprets as an effort to "leave behind the traditional stigma of therapeutic impotence to participate in the most spectacular innovations of Victorian medicine" (p. 156)

For medical historians, the book might have benefited from a longer discussion of Bucke's role in the professional politics of the period. Also the author alludes to the alienist's literary activities without providing much detail about them, for example, how Bucke met Whitman and what role he played in championing the poet's career. Otherwise, this is a fine study and a welcome addition to the medical historian's bookshelf.

Reviewed by NANCY TOMES, Department of History, State University of New York at Stony Brook, Stony Brook, New York.

MARIANNE KRÜLL *Freud and His Father.* Translated by Arnold J. Pomerans Preface by Helm Stierlin. New York and London W. W Norton, 1986 xxi + 294 pp Ill. $18 95.

Freud and His Father is a brilliant attempt to reconstruct Freud's familial origins and relationships and to place them, as well, in a historical context. Not everyone will agree with Marianne Krüll's speculative conclusions; I myself have several reservations Nevertheless, it is hard to imagine a Freud scholar who will not be fascinated and at least partly won over by Krüll's sober yet imaginative massing of the factual material from which she makes conjectures about the interpersonal dynamics in Freud's family, and particularly about the relationship between Freud and his father, Jacob

The publicity surrounding this book (it was first published in German in 1979) has focused on Krüll's argument that Freud gave up his seduction theory of hysteria in 1897 because of guilt over a "mandate" from Jacob not to look too deeply into the latter's own scandalous past. (This is quite different from the thesis Jeffrey Masson has posited about Freud's abandonment of the seduction theory, and Krüll is at pains to separate her idea from Masson's.) There has been a lot of lip-smacking speculation about what "perversions" Jacob might have committed which Freud did not want to face, and which thereby caused him to conclude that his seduction theory must be wrong But Krüll's argument—whether she is right is another matter—is not in the least scandalous, it is rather a sophisticated, careful piece of historical decoding

The following summary makes a mockery of the author's complex tapestry, but essentially this is what she suggests. Freud's father had, in various ways, communicated to his son his guilt about his past, in particular his breaking with Jewish orthodoxy, assimilating to Western ways, and indulging in sexual practices considered sinful in traditional Judaism (masturbation and/or extramarital intercourse). Jacob had also given Freud a mandate not to question him about these matters, but yet to succeed in the Western world, where the father had failed Then, in 1896, while

Freud was in the midst of investigating sexuality, his father died. This death renewed the freshness of his father's message as well as the fear of retribution that would result if Freud were not an obedient son His father's death also aroused in Freud old guilts and fears about his own childhood explorations into sexuality. Filled with guilt, Freud chose to accept his father's mandate and not look into Jacob's past but concentrate instead on his own life, thereby fulfilling in another way Jacob's wish that his son succeed

The book is bound to renew the discord that exists between historians and psychodynamically-oriented psychologists about the nature of evidence For the historian, there are two large issues First, is it sound scholarship to reconstruct the psychology of a historical character based on information gleaned from current clinical practice? Second, how accurate can one be about depicting the life of a past figure when much essential documentation does not exist? Krüll is obviously not bothered by the first question I, as a historian, am But Krull has done a remarkable job of placing Jacob Freud within his milieu Even those who object to reconstructing the life of an individual based on circumstantial information must acknowledge the plausibility of the life story she has created

We will never know all the reasons why Freud replaced his seduction theory of hysteria with the theory of instinctual infantile sexuality. Krüll's thesis is highly suggestive I disagree, however, with the conclusion she draws from it, i e., that it inevitably calls into question "the entire Oedipus theory and all concepts and constructs based upon it" (p 212) To assert this is to simplify the nature of human beings who are, after all, products of their genetic programming as well as of their environmental influences. Yet Krüll's book is valuable for its evocation of the world from which Freud came and which continued to affect him, personally and professionally She convincingly demonstrates the way traditions and attitudes are transmitted—not as primeval memories, as Freud speculated—but as parental messages and legacies, some never uttered We are reminded, once more, that both our mundane lives and our magnificent sciences are telling reflections of the men and women who shaped us

Reviewed by HANNAH S DECKER, Department of History, University of Houston, Houston, Texas.

William Osler's Collected Papers on the Cardiovascular System. Edited with an introduction by W BRUCE FYE Birmingham, Alabama: Classics of Cardiology Library, 1985 xxvii + 919 pp Ill. No price given.

This is the first in a Classics of Cardiology series for which W Bruce Fye is the editor, published in the elegant manner of the Classics of Medicine series At the beginning, the reader is provided with a rather lengthy, previously unpublished paper by Maude E. Abbott, entitled "Osler's Contributions to Our Knowledge of Heart Disease" Then follow over 900 pages of Oslerian cardiology reproduced in facsimile.

As was the case with the Classics of Medicine series, a booklet accompanies this beautifully designed book The booklet is a reprint of the paper by Charles G Roland entitled "The Palpable Osler. A Study in Survival." It is Roland's thesis that Osler's "influence continues strong especially because of his humanism, a quality that resonates clearly with the contemporary resurgence of belief in the importance of humanism in restoring art to today's scientific position" This reminds me of Henry Sigerist's comments to those of us who had the privilege of being his students in the 1940s. When addressing the question of why Osler achieved so much prominence in his day (others wrote equally good textbooks, Osler did not win a Nobel prize; and made no epochal discoveries or inventions), Sigerist expressed the view that it was because Osler fitted his day's image of what the great physician should be. He looked, acted, spoke and wrote the part His voluminous writings, both published and epistolary, had a great influence. Osler was what we would now call a role model. He was more than that, he was a legend in his own time. He worked hard at cultivating that image. Once the legend was created it had an existence of its own which was given renewed strength by Harvey Cushing's biography I do not entirely agree with Roland's idea that Osler's humanism is responsible for his continuing "popularity" because I am not certain that humanism is very popular. In the view of many, humanism is still a greatly neglected aspect of medicine

Much has been written about Osler as a specialist of one kind or another (I am guilty of writing about Osler as a medical geneticist) but the specialty to which he contributed the most perhaps was cardiology At least a fourth of his publications deal with that topic and this is a sizeable number As Maude Abbott points out, in the paper that is the foreword to the volume, Osler's bibliography contained 1420 separate communications over a fifty-year period, 1869–1919—all before the age of the word processor! This averages out at better than twenty-eight per year and many of these were lengthy books and chapters in "systems of medicine"

The tremendous impact of cardiovascular syphilis in his day is very evident in the cardiological writings of Osler. It is of interest to check Osler's understanding of the role of syphilis in disease of the aorta in light of the following quote from W G. MacCallum's classic *Textbook of Pathology* "It was impressive, and a revelation, to listen in 1903, at the meeting of the German Pathological Society, to the discussion by Chiari and others, of the discovery by Doehle and Heller of the syphilitic nature of these changes, and particularly to realize the perfectly characteristic and peculiar appearance of the aorta so affected. Since then everyone has recognized it at a glance. It seems to be one of those simple triumphs of observation that makes one ashamed to have been so blind" (17th ed., 1942, p 702). Osler, of course, is well known for his description of the bicuspid aortic valve and its predisposition to bacterial endocarditis He is also known for his linking of chorea and valvular heart disease.

A reader of this collection of Osler's writings, many of which are very difficult to come by in either the original or reprint form, will be delighted to have access to his lectures on angina pectoris and his writings on hypertension. They will be eager to see the reason why acute myocardial infarction escaped his attention as stated by Abbott in the foreword and by Osler himself: Angina pectoris was rare in hospital practice; as a consultant, he was not called to see acute cases of myocardial infarction and they did not find their way to the hospital at that stage.

In Osler's *Lectures on Angina Pectoris and Allied States* (1897), reproduced in the volume, the appendix contains an interesting letter to Osler from W T Gairdner, Regius Professor of Medicine at the University of Glasgow, concerning the pronunciation of the word *angina* Gairdner stated that he had always pronounced it with the *i* long "till Dr. Houghton of Dublin pulled [him] up . [and he] made an elaborate inquiry into the classic authorities" which convinced him that the *i* is properly short. The same appendix has an interesting and detailed account of the Matthew Arnold family which had fatal angina pectoris in three generations The angina pectoris lectures were originally prepared for delivery to the graduate class in medicine at Johns Hopkins, part of the relatively extensive post-graduate program that antedated the opening of the medical school.

Franz Messerli (*New England J Med.*, 1985, *312* 1548, *Ann. Int. Med*, 1986, *80*. 906, uses the designation *pseudohypertension* for the phenomenon and *Osler's maneuver* for the test referred to in the following statement in the section on "Arteriosclerosis" in *The Principles and Practice of Medicine* (1892, p. 668). "It may be difficult to estimate how much of the hardness and firmness is due to the tension of the blood within the vessel and how much to the thickening of the wall. If, for example, when the radial artery is compressed with the index finger the artery can be felt beyond the point of compression, its walls are sclerosed." In reading this, we have to remember that the Riva-Rocci sphygmomanometer, the first that was practical for routine clinical use, was invented in 1896 about the same time as X-rays were discovered. Messerli and his colleagues showed that persons with a positive Osler maneuver had differences of 10 to 54 mm Hg between cuff and intra-arterial pressures. Arterial compliance was lower in "Osler-positive" subjects than in "Osler-negative" subjects and correlated with the difference in pressure between the two readings.

As these samples indicate, it is difficult to imagine that any cardiologist would not be fascinated by this substantial collection of the writings 75 to 100 years ago by a (the) leading physician of the day

Reviewed by VICTOR A. McKUSICK, Department of Medicine, The Johns Hopkins University School of Medicine, Baltimore, Maryland.

T J HORDER, J A. WITKOWSKI, and C. C WYLIE (eds). ***A History of Embryology: The Eighth Symposium of the British Society for Developmental Biology***. Cambridge and New York. Cambridge University Press, 1986 xxiv + 477 pp. $99.50.

In discussing the history of psychology, the German psychologist Herman Ebbinghaus once noted that "Psychology has a long past, yet its real history is short," by which he meant that although many of the basic issues in psychology have been the subject of intellectual discussions since antiquity, scientific interest only began in earnest in the latter part of the nineteenth century The same might be said of embryology. As is well known even to embryologists who eschew history, modern experimental embryology can be traced to the work and ideas of a handful of biologists in the eighties and nineties of the last century, including most notably W. Roux and Hans Driesch By the turn of the century, embryology was firmly on its way to becoming a discipline based on experimentation. Moreover, because the same events that initiated the experimental approach also led to the final triumph of epigenesis over preformationism, by 1900 embryology had advanced to the point that E B. Wilson, in his widely read textbook of cell biology, could confidently summarize the modern view that, "every living organism at every stage of its existence reacts to its environment by physiological and morphological changes. The developing embryo

.. is a product of the response of the inherited organization to the external stimuli working upon it. If these stimuli be altered, development is altered," but Wilson wisely went on to caution that, "We cannot regard specific forms of development as directly caused by external conditions ... for the character of the response is determined not by the stimulus, but by the inherited organization .. while therefore the study of the external conditions is essential to the analysis of embryological phenomena, it serves only to reveal the mode of action of the germ and gives but a dim insight into its ultimate nature" (*The Cell in Development and Heredity* [New York, 1900], pp. 428 and 430). Contained in these quotes are allusions to most of the central issues of embryology, issues that would occupy much of the efforts of embryologists over the following decades of the twentieth century, the period that is the major focus of the book under review here.

Although there are many excellent historical treatments of the origins of embryology during the period prior to 1900, there has been no comprehensive examination of the period *following* the conceptual and experimental watersheds of the 1880s and 1890s From its all-encompassing title one might expect that *A History of Embryology* has now filled this important gap. Regrettably, this is not the case. Despite several interesting and important contributions, *A History of Embryology* is neither a comprehensive nor coherent account of embryological thought and research in the twentieth century. This is not to say that the working embryologist as well as the devoted antiquarian will not find something new and interesting in almost every chapter I only wish to point out that, the title notwithstanding, this book is not the long-awaited updated successor to Needham's *A History of Embryology*

The present book has its origins in a symposium organized by the editors and held in Nottingham, England, in April 1983 with the aim of elucidating by historical analysis the editors' thesis that present-day embryology is in a primitive state compared to other biological disciplines. To quote the editors: "World War II can usefully be taken as a watershed in the development of biological priorities affecting embryology: around that time there developed that puzzling lack of a centre, of any 'sense of progression,' that is still so much a feature of the subject" (p xi) Apart from the fact that the person cited regarding the sense of progression (Peter Medawar) is a dubious authority on embryological matters, the evidence itself, including that presented by the editors and contributors of this book, does not in my opinion support the claim that embryology has failed to keep up with progress made in other biological disciplines. It seems to me that we have learned a considerable amount about almost all aspects of embryonic development over the past forty to fifty years In fact, I would even go so far as to argue that, based on these past achievements, the prospects appear quite favorable for resolving even the most recalcitrant problems in developmental biology (e.g., gene action, pattern formation, regulation, evolution and development) by the close of the present century.

A History of Embryology is composed of fifteen chapters organized into two major subsections. The first six chapters (Part I) are devoted to major historical aspects of embryology during the period 1880–1940; Part II deals mainly with the post-World War II period and is focused on the editors' thesis of a lack of progression in the field during this time In addition, the editors have provided brief but useful introductions to several of the topics covered in Part I.

The first chapter, titled "Weismann, *Hydromedusae,* and the Biogenetic Imperative· A Reconsideration" is by Frederick Churchill He attempts to analyze the embryological work that aided Weismann in his formulation of the concept of continuity of the germ plasm. Churchill convincingly demonstrates how Weismann's formulation of this concept was instrumental in undermining Lamarckianism as well as in redefining heredity in neo-Darwinian terms, thereby leading the way to the Mendelian period.

Mark Ridley provides a fascinating glimpse into the idiosyncratic trends in British embryology between 1860 and 1930 in chapter two. Two major trends, descriptive and experimental, persisted for much of this period. Their differences can perhaps best be exemplified by textbooks written by leaders of each of the movements Whereas J W Jenkinson's *Experimental Embryology* (1909) followed the modern line established by Driesch, Roux, and Spemann, the two-volume treatise by F. Balfour on *Comparative Embryology* (1880) was in the old tradition of descriptive embryology devoted to explicating ontogeny-phylogeny relationships.

Chapter three by Jane Maienschein, titled "Preformation or New Formation—Or Neither or Both," mainly covers old ground in dealing with the American reactions to and influences on the preformation-epigenesis issue I think Maienschein draws much too sharp a distinction between some of the major contributors to this debate. For instance, A. Weismann is cast as the primary spokesman for neo-preformationist views largely because of his 1893 book *The Germ Plasm,* but his Romanes Lectures (*The Effect of External Influences upon Development* [Oxford, 1894]) also emphasized the importance of extrinsic factors of all kinds. After 1894, there was little basis for rigidly categorizing most American or German biologists as preformationists or epigeneticists

Garland Allen writes on "T. H Morgan and the Split Between Embryology and Genetics, 1910–1935" and J. A. Witkowski on "Ross Harrison and the Experimental Analysis of Nerve Growth: The Origins of Tissue Culture" in chapters four and five Although both deal with interesting and important phases in the history of embryology, these two topics have been extensively covered in previous papers and consequently there is little new information here.

The final and longest chapter in Part I is by T.J. Horder and P.J Weindling and deals with "Hans Spemann and the Organizer" Because of its length and the number of important issues it raises, this chapter deserves a separate review of its own. Regrettably, owing to space limitations, I am not able to evaluate adequately the many issues and ideas discussed by Horder and Weindling. Suffice it to say that it is an interesting and thoughtful analysis of one of the key figures in the history of embryology and is perhaps the most important contribution of this book.

The nine chapters comprising Part II are "Early Interactions between Embryology and Biochemistry," J. Brachet; "Primary Embryonic Induction in Retrospect," Lauri Saxen and S Toivonen, "Structural and Dynamic Explanations in the World of Neglected Dimensions," Robert Olby; "Form and Strategy in Biology Reflections on the Career of C H. Waddington," Edward Yoxen; "Regeneration," H Wallace, "Gradients, Position and Pattern A History," L Wolpert, "The Role of Genes in Ontogenesis—Evolving Concepts from 1883 to 1983 as Perceived by an Insect Embryologist," Klaus Sander, "Genome Function in Sea-Urchin Embryos Fundamental Insights of T. Boveri Reflected in Recent Molecular Discoveries," Eric H Davidson; and "Reductionism and Holism in Biology," Neil W Tennant

I enjoyed reading all of these chapters, especially those by Wallace, Wolpert, Sander and Davidson Nonetheless, I was left with a nagging feeling of having been titillated by brief and in some cases superficial treatments of exceedingly complex historical issues. Consequently, Part II in particular, and the entire book in general, underscores again the urgent need for a comprehensive analysis of embryological progress in this century. Although I can recommend this book to anyone with an interest in embryology and the history of biology, I still eagerly await the book that can deservedly be titled *A History of Embryology*

Reviewed by RONALD W. OPPENHEIM, Department of Anatomy, Bowman Gray Medical School, Wake Forest University, Winson-Salem, North Carolina

SHARON E. KINGSLAND. ***Modeling Nature: Episodes in the History of Population Ecology.*** Chicago and London. University of Chicago Press, 1985 ix + 267 pp Ill $27 50

When I started to teach the history of ecology at Johns Hopkins in 1972, I had to rely at first on a rather scanty secondary literature Besides the somewhat outdated 60-page historical section in the *Principles of Animal Ecology* of W C Allee and his collaborators (1949), there were forty to sixty uneven articles scattered mainly in biological journals, and the pioneering studies of Frank Egerton, which heralded an approach in better agreement with the standards of professional history of science There has since been a wave of significant contributions, such as those of Ronald Tobey and Eugene Cittadino on American plant ecology, of Douglas Weiner on ecology and conservation in the U.S.S.R., or the recent survey by Robert McIntosh Indeed, fifteen years after my uneasy beginnings, on reading this new book by Sharon Kingsland, (now teaching at Hopkins herself), I feel that the history of ecology has come of age

As its subtitle makes clear, *Modeling Nature* is not intended as a comprehensive history of population ecology The author has chosen rather to analyze key elements and to reconstruct crucial episodes in the development of theoretical population ecology from 1920 to 1970 Though chronological, the study is not a simple linear narrative. The episodes Kingsland has chosen to investigate reveal how researchers coming from various philosophical and scientific backgrounds, engaged in diverse, and at times divergent endeavors, and gave a new shape to thinking about population The early attempts at population modeling were initiated outside the mainstream of ecology, and were often perceived with indifference or hostility by the new professional ecologists educated and rooted in the descriptive field or laboratory biology traditions. What the book unravels very successfully is the complex process, involving bitter debates, conflicts and schisms, by which mathematical modeling moved from the fringe to the core of population ecology.

This is an adventure story of the development of ideas such as the logistic curve of population growth and the *r* and K parameters, which are now part of the elementary education of any population ecologist It is a serious book, but written with outstanding clarity and pedagogical skill, so that it can be read with pleasure, understanding, and profit, even by the most obdurate qualitative minds

In the first decades of this century, population ecology had developed on two levels: "autecology" studying the relationships of the organism with its environment, and "biocoenology," focusing on the distribution and evolution of natural associations of species Though these studies were essentially descriptive, ecologists were not immune to quantification, but limited to simple statistics

Alfred Lotka's ambitious *Elements of Physical Biology,* published in 1925, did not emerge from this disciplinary framework He was a scientific amateur whose contention was that "biologists have been in the habit of reflecting upon the evolution of individual species" and that "this point of view does not bear the promise of success (p. 36)" Lotka's goal was to embrace the entire world as a system, adopting the approach of the physical chemists in their analysis of chemical systems. His book encompassed a treatment of evolution as the irreversible transformation of the earth system, man included It dealt with chemical and nutrient cycles, energy and matter exchanges, population growth, prey-predator relationships, as well as the origin and function of consciousness Its approach was foreign to ecology, its scope much broader Lotka's physicalist program, largely inspired by the Spencerian commitment to a synthetic philosophy, was following the suggestions of Ludwig Boltzmann who, as early as 1886, had advanced the idea that organisms are struggling for energy, and of Ostwald, who saw energy as the central organizing concept of the physical and biological sciences

To his disappointment, Lotka did not get much attention from physicists, it was from Raymond Pearl, professor of biometry and vital statistics at Johns Hopkins, that support and publicity for his ideas came. Pearl was not an ecologist either, he was first and foremost a demographer, interested in life tables, death rates, survivorship curves and population growth. In 1920, with his collaborator Lowell Reed, he presented the logistic curve in which he saw the explanatory law of population growth Until his death in 1940 he remained convinced of the validity of his claim and was embroiled in unceasing controversies.

Coming into Pearl's orbit, Lotka could not ignore the "furor over the logistic curve" However, he did not see in the logistic curve an explanation, but a mere empirical statement, or as he aptly characterized it, "an animated question mark" (p 83), a useful starting point for the mathematical treatment of population phenomena The future was to prove him right

By the 1930s mathematization was proliferating and theoretical population ecology was not restricted to the use of the logistic curve Sir Ronald Ross had complemented his earlier malaria researches with the epidemiology of the mosquitoes transmitting the disease. William Robin Thompson had investigated the cycles of host-parasite populations. From 1925 on, the great Italian mathematician Vito Volterra had started a series of very influential contributions on the fluctuations of prey and predator populations. In 1933, the Russian geophysicist V A. Kostitzin published his mathematical treatment of symbiosis and parasitism, whereas the Australian entomologist Alexander Nicholson, assuming a balance of nature, presented his theory of population regulation based on the operating mechanism of density-dependent intraspecific competition

It is clear that ecologists were then confronted with what Kingsland calls "a glut of theories" (p 126) These often involved arduous mathematics that ecologists were ill-trained to absorb Moreover the most sophisticated models, developed by non-biologists, were rightly seen as biologically naive, it was all too easy to discard them as mathematical figments. There was among ecologists a consensus that theorizing had gone far beyond observation and experiment And when experiments came, "verification was not always forthcoming " In fact, well into the 1960s the very utility of modeling was questioned by a great many ecologists.

Theoretical population ecology found its second wind in the works of Evelyn Hutchinson and his school Their central design was to understand how species coexist. Their enterprise placed strong emphasis on the significance of the principle of competitive exclusion worked out by the Russian biologist Georgii Frantsevich Gause in the 1930s

Combining mathematical modeling, laboratory testing and field studies, Gause had shown the necessity of extinction if two species competed for the same energy source in a highly simplified microcosm, but also—in agreement with Lotka's contention—that their coexistence remained possible if territory and resources were apportioned. Relating competitive exclusion to the ecological niche, Gause stressed the idea that the task of ecology was "to account for the spatial and temporal relations of the community" (p 158). In this context, the function of mathematical models was not to predict specific events, but rather to unravel the principles governing community structure.

Hutchinson's starting point was to work at a theoretical redefinition of the niche concept His most famous disciple, Robert MacArthur, initiated a new approach to modeling in ecology. Sacrificing mathematical precision to gain biological realism and generality, MacArthur made possible qualitative comparison between types of organisms and types of environment. The identification of patterns became the chief tool of the population ecologist Moreover, following David Lack, who had used the competitive exclusion principle to give an evolutionary explanation of the coexistence of closely similar species of birds in the Galápagos,—Darwin's finches—MacArthur and his collaborators Richard Levins and E. O. Wilson joined ecology and population genetics—which had played a crucial role in the emergence of the new Darwinian synthesis—to create the new population biology of the 1960s

No success, however, is ever undisputed and Kingsland concludes her analysis by underlining the sharp polarization and the battle line drawn at the turn of the 1970s The history of science cannot be equated with peaceful accumulation of evidence; it is an essentially polemical process and it is with sober realism that the author concludes her last chapter by raising the question "How many schools of ecology can peacefully coexist?" Indeed her book is not only epistemologically sophisticated, it is also sociologically perceptive. Throughout the book one comes across hints that an ecology of population ecologists is possible, nay, necessary, and that the acute forms that conflicts take at times in science are to be explained by competition for reputation, power and resource allocation

This first book of Sharon Kingsland is an impressive achievement.

Reviewed by CAMILLE LIMOGES, Centre pour l'évaluation sociale des technologies, Université du Québec, Montréal, Canada

DANIEL M. FOX. ***Health Policies, Health Politics: The British and American Experience, 1911–1965.*** Princeton, New Jersey Princeton University Press, 1986 xi + 234 pp. $25 00

Daniel Fox confronts us with bold affirmations of strongly held personal opinions in this interesting and provocative book While others may have doubts regarding perplexing concepts as to how and why the movement toward a national health program in the United States went off the rails earlier in this century, or why the National Health Service in the United Kingdom has stalled, Fox has none. In part, this is because of his thesis, which rearranges the customary emphasis on politics, castigating social reformers while commending the medical profession. In part, rhetorical flourishes aside, it is because he sees previous interpretations as "historicism"· inventions which undergird, in Fox's words, "belief that there is a purpose in history which it is our professional duty to discover and communicate" (D M Fox, "The decline of historicism," *Bull. Hist Med.,* 1983, *57:* 596–610).

For Fox, the previously unrecognized motivating factor in health policy development was "hierarchical regionalism," a group of assumptions which holds that "... 1) The causes of [*sic*] cures for most diseases are usually discovered in the laboratories of teaching hospitals and medical schools 2) These discoveries are then disseminated down hierarchies of investigators, institutions and practitioners who serve particular geographic areas 3) Health policy should stimulate the creation of hierarchies in regions that lack them and make existing ones operate more efficiently" (p ix) This has operated to block appropriate social action in the health field in the United States since the turn of the century; and in the same period shaped the British National Health Service in its formation and now in its static phase

Fox does not lay the failure of the United States to enact compulsory health insurance at the door of organized medicine and organized labor, nor does he see America's entry into the First World War as the source of the abrupt termination of legislative efforts. While Fox agrees that the leaching out of Progressive Era principles initiated the failure of much social reform in America, he argues that in the medical arena it was a change of goals rather than a weakening of intention Economists and obdurate, inept reformers, rather than cunning and greedy physicians, ended America's essay into a national health program.

So far as the United Kingdom is concerned, the case is equally clear to Fox. A coalition of politicians, civil servants, insurance industry leaders, business executives and professionals, along with representatives of organized medicine were able to finesse the radical reformers earlier, and so the country achieved the national health program that eluded American efforts The problems raised by their success, however,—the emphasis on costly curative medicine and expensive technical medical facilities and equipment (also a problem in the United States)—have created a national financial crisis and stalled movement toward real advances in health care

Fox may have a point in chastising reformers for their naiveté in holding to the notion that medical science will offer an endless cornucopia of marvelous discoveries to end disease and suffering, and their arrogance in denying professionals, like the physicians involved, a significant role in health policy-making He may also justifiably fault economists (as he did at greater length in "From reform to relativism," *Milbank Mem Fund Quart. Health and Society,* 1979, *57.* 297–335) for failing to maintain the line between advocacy and objectivity But he is attacking their roles only incidentally in this reinterpretation of the unfolding of national health policies in the twentieth century. His main purpose is driving the historicists out of the temple of historiography. "I am a critic of . the belief that there is a purpose in history, that events in the past unfold toward the present, and that history teaches" (p x) Or further, "In both a practical and philosophical sense, however, there is no past—no correct description of an earlier time. There is only evidence, which historians must reinterpret continuously. The study of history is a source of experience, not of justifications for policy" (p. 212) In short, not only do we learn from history that we do not learn from history, but we *should* not learn from history! Here fewer readers will follow.

This is a well thought out, scholarly book which belongs on the reference shelf along with the other analytic texts Fox condemns. In a way, by putting a different complexion on the details of the growth of health and medical care policies in the United States and United Kingdom in this century, Fox may have done his theme a disservice. He has shown that history can be successfully interpreted for the understanding of current events, and perhaps even as a useful guide in redirection for the future!

Reviewed by **GEORGE A. SILVER,** Institution for Social and Policy Studies, Yale University, New Haven, Connecticut

VICENTE NAVARRO *Crisis, Health, and Medicine: A Social Critique.* New York and London: Tavistock Publications/Methuen, 1986. vi + 281 pp. $29 95 (cloth), $12 95 (paperbound)

Navarro collects seven papers that he wrote between 1978 and 1984 in order to demonstrate the superiority of Marxism over the "methods traditionally used in the various disciplines in social science" (p 3) for explaining the history and present condition of medicine The crisis of his title is a result of the "heightening of the internal contradictions of our Western societies in all their economic, political, and civil dimensions and in all areas of life (including health and medicine)" (p 2). According to Navarro, the crisis of the "Western system of medicine" is characterized by "growth and ineffectiveness" as a result of "*needs created by the process of capital accumulation* on the one hand and the *demands expressed by the working population* on the other" (p 25, his italics) These needs and demands are "translated into the *daily practice of the class struggle*" (p. 25) The troubles of the health sector are not, therefore, the result of the behavior of interest groups, and especially not of "manipulation" by the medical profession (p 25)

The strengths of the book are the rigor of Navarro's arguments and the often engaging vehemence with which he presents them Moreover, when he bases his writing in part on primary sources—for example, in the paper on "Work, Ideology and Science"—he contributes to our knowledge of contemporary history

The weakness of all of Navarro's publications from the point of view of historians of medicine is his belief that communities of scholars are defined only by ideology He assumes that any challenge to his data and interpretations is ideologically motivated Moreover, he insists that any criticism of Marxist scholars—except his own—is evidence of their continuing persecution in the United States. Thus he ignores the historical literature of the past decade and continues to cite approvingly scholarship by Marxists that has been sharply criticized for loose handling of evidence.

Similarly, he regards himself as the sole judge of his methodology For example, he apparently believes that it is appropriate to stop reading primary sources about a subject when he has found a few documents that seem to support his thesis that the class struggle is the engine of history As a result, he makes errors of fact and rips events from their historical context Examples of his technique and its results abound in his discussion of the British National Health Service in the paper on "The Western System of Medicine" Navarro's factual errors betray his inattention to both primary sources and other historians For instance, his assertion (p 31) that the "nationalization of the main components of the health sector" was a "victory" for the "British working class" ignores advocacy of nationalization by prominent physicians and surgeons, and antagonism to it among leading socialists Just as astonishing is his claim that Lord Dawson—who never held office—was the "first minister of Health" under Lloyd George (p. 45).

These errors, and many others in the book, are not the slips of memory and transcription that afflict most historians Because Navarro links each of his "facts" to a bold interpretation, his errors suggest a fundamental disrespect for the enterprise of history as most of us practice it

Navarro's paper attacking Paul Starr's *The Social Transformation of American Medicine* (pp. 185–211) could become a classic cautionary tale for historians who are committed to archival research and to debate among peers about the interpretation of data Starr and Navarro have more in common than either would admit they are both impatient with primary, and especially manuscript, sources, and both have avoided the intellectual company of historians who work in the history of health and medicine. Their differences of interpretation are indeed· profoundly ideological Navarro has found his proper antagonist.

Reviewed by DANIEL M FOX, Health Sciences Center, State University of New York at Stony Brook, Stony Brook, New York

RASHI FEIN ***Medical Care, Medical Costs: The Search for a Health Insurance Policy.*** Cambridge, Massachusetts, and London Harvard University Press, 1986 viii + 240 pp. $20 00

Rashi Fein, a distinguished medical economist, has written a concise and cogent analysis of the history, present status, and future of the American health care insurance system

The book begins with a history of programs to insure individuals against the costs of hospitalization The first, Blue Cross, was developed and run by hospitals, and has been primarily concerned with hospital income through direct payments to hospitals and broad coverage of the population. Private hospital insurance, which soon followed, indemnified individuals against hospital costs and selected low risk individuals to insure During World War II, health insurance became largely employment-based, exacerbating the problems of those not in the labor force or in jobs lacking health insurance. Fein traces the history of national health insurance proposals up to the enactment of Medicare and Medicaid in 1965

Fein's historical analysis fails to describe changes in health care over this period It also disregards state programs, the Hill-Burton Act of 1946, which supported hospital construction with federal funds, and the Veterans Administration hospital system, where most patients have been treated for conditions unrelated to their military service It might be noted that after World War II, the American Medical Association favored treating veterans in community hospitals at government expense Had such a program been administered by the Veterans Administration, it would have established a precedent that might have completely altered Medicare

The second part of the book is an analysis of the current system. Fein notes the gaps in Medicare coverage, the growing co-insurance costs to the insured, and the refusal of many physicians to accept assignment. The use of Blue Cross as the primary Medicare intermediary has put the monitoring and auditing of hospital costs in the hands of organizations controlled by hospitals, with the expected results. Fein concludes that Medicare survives rather than flourishes

The condition of Medicaid is far more dismaying. As a joint program of the federal government and states, it has suffered from wide state-to-state variations in coverage and benefits. In 1983 Medicaid covered only 40 percent of households with incomes below federal poverty standards. Fein concludes: "Programs for the poor tend to become poor programs. The needy are better served by universal programs in which their fate and that of other Americans are inexorably intertwined" (p. 123)

Fein favors health maintenance organizations as a useful reform However, many HMOs no longer give physicians a financial incentive to keep members healthy In order to provide coverage over a wide geographical area, HMOs have contracted with private physicians on a fee per patient-visit basis These physicians have no stake in lowering the HMO's costs

At this point Fein describes the problems confronting the nation's health care system In 1979 there were 35 million uninsured persons and millions more with inadequate coverage The federal government has turned to cost-cutting and imposed a greater financial burden on the insured. Classic economic market principles have been proposed to reduce health care costs Their main effect will be to reduce the care available to the poor

Fein's solution to the dilemma between cost control and equity (health care provided on the basis of need) is a system of national health insurance In order to gain political acceptance, the system should be funded by the federal government and administered by the states, with all the variations among states that this implies The history of Medicaid will deter many from accepting this proposal Furthermore, states, as employers, are among the nation's largest purchasers of health insurance. Observation suggests that they have failed to exert much initiative or responsibility here either.

Fein's book, although it does not break new ground, can be highly recommended to those seeking an overview of a vital issue described in a dispassionate and forthright manner The lucid analysis and pleasing writing style make it an excellent choice for advanced undergraduate or graduate courses, especially to show how contemporary problems are shaped by historical experiences.

Reviewed by WILLIAM G. ROTHSTEIN, Department of Sociology, University of Maryland Baltimore County, Catonsville, Maryland.

BENNETT M. JUDKINS *We Offer Ourselves as Evidence: Toward Workers' Control of Occupational Health.* New York-Westport, Connecticut-London: Greenwood Press, 1986 (Contributions in Labor Studies, no 19). xii + 249 pp $35.00

Judkins, a sociologist, has written a competent history of social action in response to impaired breathing by workers in the American coal mining and textile industries and their political allies. His account of the brown lung movement is one of the first by a scholar and, despite its limitations, is a useful contribution to contemporary.

history. However, for the black lung movement, on which much more research has been done, he cites rather than comprehends major secondary sources and pays superficial attention to important primary sources

Black and brown lung are not diseases, according to contemporary definitions, but rather labels for respiratory distress among workers in coal mines and cotton textile factories. Efforts to make social policy to alleviate these conditions creates medical as well as industrial conflict Black lung is a polemical label for pulmonary distress among coal miners, whether or not there is clinical evidence of lung damage or whether, at autopsy, pathologists diagnose coal miners' pneumoconiosis or progressive massive fibrosis. Brown lung, even when it is called byssinosis, is apparently indistinguishable, both clinically and at autopsy, from chronic obstructive pulmonary disease

As a result of political action by coal miners and their allies, the United States Congress, acting contrary to most medical advice, wrote legislation in 1969 and 1972 defining the symptoms that the miners called "black lung" as a disease whose victims were eligible for federal disability payments. The brown lung movement was less effective in pressing its case at the federal level, for reasons which Judkins presents

Although Judkins knows that black and brown lung are the conditions of our time whose social construction is least controversial, he repeatedly writes as if they are readily discernible biological phenomena. Like many of the participants in the social movements he describes, he implies that anyone who opposed federal compensation for suffering workers was acting only on the basis of economic self-interest or class loyalty. He makes pathologists and epidemiologists who were working within the accepted rules of their disciplines into allies of the owners of mines and factories and their company physicians Not only has he misread primary sources, but he did not read carefully enough several secondary sources, including a paper published in this journal which he cites approvingly even though it contradicts his interpretation

This book also exemplifies the barriers that separate scholars who use historical methods, but identify with different disciplines Had Judkins been familiar with how political scientists use historical analysis, he might have examined the administration of black lung benefits in the context of Social Security Disability Insurance. In particular, he would have understood the role of the states in certifying workers as disabled and therefore eligible for federal benefits. Had Judkins been familiar with how historians struggle with problems of narrative, he might have realized that he had selected and arrayed his data to yield the theoretical conclusions he draws, as if from an experiment, at the end of the book

In sum, Judkins has written a helpful, but flawed, book. Because he wants to tell his story from the perspective of sick and angry miners and textile workers, he oversimplifies the complex relationship between illness and disease in medicine and public policy in the late twentieth century

Reviewed by DANIEL M. FOX, Health Sciences Center, State University of New York at Stony Brook, Stony Brook, New York

HARRIS B SHUMACKER, JR. ***Leo Eloesser, M.D.: Eulogy for a Free Spirit.*** New York· Philosophical Library, 1982. 483 pp. Ill. $29 95.

As a child he signed his letters "Little Leo," and as an adult he still was not tall, but as a surgeon, teacher, and humanitarian Leo Eloesser was truly a giant among those who knew him Born of German-Jewish parents in 1881 in San Francisco, Eloesser achieved fame in surgery far beyond the bounds of his native city, where he taught and practiced for many decades. His adventures in Spain and China, and his final years in Mexico, are told here in loving detail by a fellow surgeon, but there are also many long sections from Eloesser's own writings so that his voice may be clearly heard He was a friend of many great musicians and artists, and an accomplished performer himself. Thus his life was incredibly enriched. All of this is well described in the book, plus his student days in Germany, the surgical environment in California after his return in 1909, and conditions in China immediately after World War II.

Shumacker's well-crafted biography is more than mere history of medicine and surgery. It is an adventure story that merits both praise and wide reading. The ninety-five years of Leo Eloesser's life deserve this book, written and compiled by a friend who can spot a good story and knows how to tell one. This is an example when a short review cannot hope to do justice to a long book.

Reviewed by **GERT H. BRIEGER**, Institute of the History of Medicine, The Johns Hopkins University, Baltimore, Maryland

ARTHUR KLEINMAN and BYRON GOOD (eds) ***Culture and Depression: Studies in the Anthropology and Cross-Cultural Psychiatry of Affect and Disorder.*** Berkeley-Los Angeles-London: University of California Press, 1985 (Comparative Studies of Health Systems and Medical Care). xi + 535 pp. $45 00.

Here is another book about "depression." It sometimes seems that practically nobody defines depression before he starts writing about it. In this book there are twenty-two contributing authors including the two editors, thirteen chapters, a preface, an introduction and an epilogue. Only one of the chapters has a historical orientation, the one by Stanley W. Jackson, titled "Acedia the Sin and its Relation to Sorrow and Melancholia " The remaining papers deal with a hodge-podge of "depression" in various cultural and linguistic groups. The distinctions between psychological, psychiatric (medical), anthropological and sociological frames of reference are foggy It is sad (depressing?) that even the orientations of the medical authors towards "depression" may be confused and inaccurate. Adherence to controversial nosologies and nomenclatures, psychology tests, rating scales, questionnaires and inclusion of irrelevant data has set adrift many of the essays There is a singular lack of scientific specificity and of historical-bibliographical methodology, a lot of pseudo-scientific verbiage and certainly much clinical inaccuracy Paperbound methods are of limited importance as compared with those of classical, orthodox clinical medicine which are firmly based on medical and sociological history, psychobiological dynamics, pathology, and psychopathology, including as much observational evaluation by the physician as possible, along with laboratory backup as available.

Few of the authors in this volume seem to be clear about what they are writing.

The reader has to discern that it is abnormal mental depression they are focusing on and this has a range of meanings that cannot be ignored in the discussion. The language of "feeling" is complicated, controversial and difficult to understand, but surely we must be disciplined to be as exact as possible when elaborating scientific data. The words "affect and disorder" in the subtitle of this book are certainly appropriate—there is plenty of disorder

Nosologies and nomenclatures change over the years, for better and sometimes worse, but we must at least always try to be as accurate and specific as possible with "words." A lot of the confusion results from indistinct limits between the academic disciplines—philosophy, history, psychology, sociology, anthropology, total biology, and so on Medicine is something apart. By tradition, medicine deals with abnormal human biology and the treatment and prevention of illness. Unfortunately, not all medical writers stay within the limits of their expertise, nor do those from the other disciplines Interdisciplinary frames of reference and pseudo-science from outside of medicine may confuse and dilute data rather than assist clear thinking

It would appear that many of the authors of this book have not taken full account of the complexities of "culture and depression" Apart from the contribution by Jackson noted above, the book as a whole was a disappointment to this reviewer.

Reviewed by EDWARD L. MARGETTS, Emeritus Professor of Psychiatry, University of British Columbia, Vancouver, B.C., Canada

BOOK NOTES

VICENTE COELHO DA SILVA SEABRA. ***Elementos de chimica*** (facsimile) Coimbra, Portugal: Departamento de Química; Universidade de Coimbra, 1985. xviii + 485 pp. $10.00 (paperbound)

This is a facsimile of the work *Elementos de chimica* (part 1, 1788, part 2, 1790) of Vicente de Seabra It has been published by the Chemistry Department of the Faculty of Sciences and Technology of the University of Coimbra in Portugal to commemorate the second centenary of its original printing Seabra was a Brazilian who obtained his medical degree at the University of Coimbra and who, after being a demonstrator and lecturer, became Substitute Professor of Chemistry at the university in 1801 Seabra was concerned to develop a textbook for students of chemistry and was knowledgeable about the many developments in chemistry in the latter part of the eighteenth century. The Portuguese text is accompanied by short introductions in both Portuguese and English

CHARLES E RAVEN ***John Ray: Naturalist*** (reprint of 2d edition, 1950). Cambridge and New York. Cambridge University Press, 1986 (Cambridge Science Classics series) xxv + 506 pp $24.95 (paperbound)

First published in 1942 and reissued in 1950 with two pages of additional notes, Charles Raven's biography of John Ray appears now in paperback with a five-page account of the author's life (George Raynor Thompson first reviewed it in *Bull. Hist. Med.,* 1943, *14:* 530–31). Raven's account concentrates on the scientific and scholarly works of Ray because there are few traces of his personal life left for a biographer. Although "botany was the field of Ray's greatest and lifelong interest, it is by no means his only or even perhaps his chief claim to the gratitude of posterity" (p. 308). A gifted ornithologist, entomologist, ichthyologist, and theologian, his works on these subjects had much influence, especially on the natural theology of the eighteenth and nineteenth centuries. Ray also published fundamental works in folklore and on English dialects. Raven presents all these facets of Ray's complicated life in rich detail while maintaining balance and proportion among Ray's varied achievements. This is, indeed, a classic biography of one of Great Britain's most important scientists.

CARL J. PFEIFFER. *The Art and Practice of Western Medicine in the Early Nineteenth Century.* Jefferson, North Carolina: McFarland & Co., 1985. xiv + 238 pp. Ill. $29.95. (Order from McFarland & Co., Inc., Publishers, Box 611, Jefferson, North Carolina 28640.)

This 238-page book contains twelve chapters on early nineteenth-century medicine and related subjects. Physiology receives the greatest attention in chapters on galvanism, cardiovascular physiology, gastroenterology, and obstetrics. Therapy is examined in chapters on leeches, vaccination, cold water and blisters, and the general materia medica. Another chapter describes perceptions about the role of weather and the moon in disease. Two socially-oriented chapters concern relevant European developments during the period, and American medical practice and the medical profession. The book uses numerous quotations from medical and scientific sources of the period, most of which are by English authors, and is illustrated.

Daedalus: Journal of the American Academy of Arts and Sciences, Spring 1986, vol. 115, no. 2, *America's Doctors, Medical Science, Medical Care.* Cambridge, Massachusetts: American Academy of Arts and Sciences, 1986. x + 232 pp. $5.00 (paperbound).

Historians of medicine with an interest in contemporary medical affairs will find this collection of essays a mine of information. More important, one may also observe current medical leaders pondering the issues of the present and the future in the light of the past as they have perceived it. This is an unusual issue of the journal *Daedalus* in that it began as a *festschrift* for Walsh McDermott, one of this century's most perceptive medical teachers. The volume is clearly a labor of love by one of McDermott's most successful students, the physician David Rogers.

It includes articles about new views of disease by Eric Cassell; the physician-scientist by Edwin Kilbourne; and medical education by Robert Ebert, Robert Petersdorf, and Paul Beeson. Other articles describe cost issues (Robert Blendon); and medicine and society (Leighton Cluff and Kenneth Johnson). Kurt Deuschle discusses cross-cultural medicine using the Navaho Indians as a case study. The volume opens and closes with articles by David Rogers on the context in which Walsh McDermott began his career and on what the future may hold.

Manuscripts of the Dibner Collection in the Dibner Library of the History and Science of Technology of the Smithsonian Institution Libraries. Washington, D.C.: Smithsonian Institution Libraries; Canton, Massachusetts: Watson Publishing International, 1985 (Smithsonian Institution Libraries Research Guide, no. 5). xiii + 145 pp. Ill. $35.00

The Dibner manuscript collection is a dazzling miscellany of manuscripts dating from the Middle Ages to 1980 and dealing with technology, the physical sciences, mathematics, and (to a lesser degree) biology and medicine. Some of the more substantial and interesting medical manuscripts are a sixteenth-century Italian receipt book (no. 10); a fifty-page manuscript by William Cullen on the brain and nervous system (no. 214); letters and drawings by Louis Pasteur (nos. 916 and 965); and two large collections of materials by and about Wilhelm Conrad Röntgen (nos. 1128 and 1423). Three Pasteur items and an X-ray by Röntgen are illustrated. A lucid foreword explains how the catalogue was constructed, and a detailed index of authors and subjects provides quick and easy access to the entries. The typography and design are at once handsome and practical.

THE BULLETIN RECENTLY RECEIVED*

G. C Ainsworth *Introduction to the History of Medical and Veterinary Mycology.* Cambridge and New York: Cambridge University Press, 1986. xi + 228 pp. Ill. $49.50

Mitchell G Ash and Ulfried Geuter (eds) *Geschichte der deutschen Psychologie im 20 Jahrhundert. Ein Überblick.* Opladen, W. Germany: Westdeutscher Verlag, 1985 386 pp. No price given (paperbound)

Ronald Bayer, Daniel M. Fox and David P. Willis (eds.) *AIDS· The Public Context of an Epidemic,* vol. 64, suppl. 1, 1986, to the *Milbank Quarterly.* New York: Milbank Memorial Fund; Cambridge University Press, 1986 182 pp. $10.00 (paperbound)

Peter Beighton and Greta Beighton. *The Man Behind the Syndrome.* Berlin and New York: Springer Verlag, 1986. 240 pp Ill. $36.00

Tommy Bengtsson, Gunnar Fridlizius and Rolf Ohlsson (eds). *Pre-Industrial Population Change. The Mortality Decline and Short-Term Population Movements* Stockholm: Almqvist & Wiksell International, 1984 419 pp. SK280,00.

Pia Bennike. *Palaeopathology of Danish Skeletons A Comparative Study of Demography, Disease and Injury* Copenhagen: Akademisk Forlag, 1985 272 pp Ill DK195,20 (paperbound)

Boletín mexicano de historia y filosofía de la medicina, vol 9, 1986. Mexico City· Sociedad Mexicana de Historia y Filosofía de Medicina, 1986. 105 pp. $10.00 (paperbound).

Åke Boman *Sjukvården vid Västerås lasarett, 1786–1893* [The Care of the Sick at Västerås Hospital, 1786–1893]. Stockholm: Museum of Medical History, 1986 (Supplement to *Nordisk Medicinhistorisk Årsbok,* no. 12). No price given (paperbound).

Charles Bonnet and John Turberville Needham. *Science Against the Unbelievers The Correspondence of Bonnet and Needham, 1760–1780* Edited by Renato G Mazzolini and Shirley Roe Oxford, England: Voltaire Foundation at the Taylor Institution, Oxford, 1986 (Studies on Voltaire and the Eighteenth Century, no 243). xix + 409 pp. Ill. $80.00

Roger J Bulger (ed) *In Search of the Modern Hippocrates* Iowa City. University of Iowa Press, 1987. 256 pp $27 50

Vern Bullough and Bonnie Bullough *Women and Prostitution: A Social History* Rev ed. of *Women and Prostitution: An Illustrated Social History.* Buffalo, N Y.. Prometheus Books, 1987 xvi + 374 pp $14.95 (paperbound).

J. A. Burrow. *The Ages of Man: A Study in Medieval Writing and Thought* Oxford and New York: Oxford University Press, 1986 ix + 211 pp Ill $39.95

Walter B Cannon. *The Mechanical Factors of Digestion* (reprint of 1911 edition). With an introduction by Horace W Davenport Canton, Mass.: Science History Publications U.S.A., 1987. xii + 195 pp. $20 00

Martin Cherniack. *The Hawk's Nest Incident. America's Worst Industrial Disaster.* New Haven and London. Yale University Press, 1986. x + 194 pp. Ill. $19 95.

William Coleman. *Yellow Fever in the North: The Methods of Early Epidemiology*

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*The *Bulletin* reserves freedom of decision as to the publications to be included in this section Items received, other than those reviewed, are ultimately incorporated into the collection of the Institute of the History of Medicine

Bull Hist Med. 1987. *61* 310–313

Madison, Wisconsin, and London University of Wisconsin Press, 1987. xvi + 202 pp Ill $49 50 (cloth); $17 50 (paperbound)

William Cooper. *William Cooper's "A Catalogue of Chymicall Books," 1673–88: A Verified Edition* Edited by Stanton J Linden New York and London: Garland Publishing, 1987 liv + 159 pp Ill $37 00

Jean-Nicolas Corvisier *Santé et société en Grèce ancienne* Preface by Jacques Dupâquier Paris. Economica, 1985 200 pp F125,00 (paperbound)

Alfred W. Crosby *Ecological Imperialism. The Biological Expansion of Europe, 900–1900.* Cambridge and New York. Cambridge University Press, 1986. xiv + 368 pp. Ill. $22 95.

John Cutting and M Shepherd (eds). *The Clinical Roots of the Schizophrenia Concept· Translations of Seminal European Contributions on Schizophrenia.* Cambridge and New York: Cambridge University Press, 1987 vi + 238 pp. $54.50.

Horace W. Davenport *Fifty Years of Medicine at the University of Michigan, 1891–1941* Ann Arbor, Mich: University of Michigan Medical School, 1986 525 pp. No price given.

Deborah Dwork *War is Good for Babies and Other Young Children. A History of the Infant and Child Welfare Movement in England, 1898–1918* London and New York· Tavistock Publications; Methuen, 1987. 307 pp. Ill $37.00.

David Eisenberg with Thomas Lee Wright. *Encounters with Qi. Exploring Chinese Medicine* New York Penguin Books, 1987. 254 pp. Ill. $6.95 (paperbound)

Karl Enigk. *Geschichte der Helminthologie im deutschsprachigen Raum* Stuttgart and New York. Gustav Fischer, 1986 xviii + 356 pp Ill. DM58,00.

Michel Foucault. *Dream, Imagination, and Existence,* translated by Forrest Williams, and Ludwig Binswanger, *Dream and Existence,* translated by Jacob Needleman, in *Dream and Existence,* special issue of *Review of Existential Psychology and Psychiatry,* vol 19, no. 1, 1986 107 pp. $10.00

Stephen Frankel *The Huli Response to Illness.* Cambridge and New York. Cambridge University Press, 1986 (Cambridge Studies in Social Anthropology, no 62). xvi + 201 pp. Ill. $44 50.

Catherine Gallagher and Thomas Laqueur (eds). *The Making of the Modern Body Sexuality and Society in the Nineteenth Century* (originally published as *Representations,* no. 14, Spring 1986) Berkeley-Los Angeles-London. University of California Press, 1987 xv + 242 pp. Ill $35.00 (cloth), $9.95 (paperbound).

Robert Mortimer Gascoigne (comp). *A Historical Catalogue of Scientific Periodicals, 1665–1900, with a Survey of Their Development.* New York and London: Garland Publishing, 1985 xii + 205 pp $27.00

Robert Germain. *Le mouvement infirmier au Québec Cinquante ans d'histoire* Montreal: Editions Bellarmin, 1985. 565 pp. Ill. $Can. 20 00 (paperbound).

Mary Gibson *Prostitution and the State in Italy, 1860–1915* New Brunswick, N.J., and London. Rutgers University Press, 1986 xi + 297 pp $38.00

Penina Migdal Glazer and Miriam Slater. *Unequal Colleagues The Entrance of Women into the Professions, 1890–1940* New Brunswick, N.J, and London: Rutgers University Press, 1987. xii + 295 pp Ill $9 95 (paperbound).

Anita Guerrini (comp.) *Natural History and the New World, 1524–1770. An Annotated Bibliography of Printed Materials in the Library of the American Philosophical Society.* Philadelphia· American Philosophical Society Library, 1986 (Library Publication no 11) xi + 83 pp. $5.00 (paperbound)

David Hamilton *The Monkey Gland Affair* London: Chatto & Windus, 1986. xvi + 155 pp. Ill. £11.95.

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CONTRIBUTORS TO THIS NUMBER

THEODORE M. BROWN is an associate professor in the Departments of History and Preventive Medicine at the University of Rochester, River Campus Station, Rochester, New York 14627. His research is currently concerned with the development of psychosomatic medicine in the United States from 1890 to 1945

LUIS GARCÍA-BALLESTER, a professor at the Institute for Medieval Studies (History of Medicine) of the scientific research council of Barcelona (CSIC), works primarily on Galen, Galenism, and the social history of medicine before the Scientific Revolution. He is presently investigating the intellectual and social history of medicine in late medieval Spain in the context of Western Mediterranean countries His address is Instituto de Estudios Medievales "Milà i Fontanals," CSIC, Egipciacas 15, 08001 Barcelona, Spain.

CHRISTOPHER LAWRENCE is a senior lecturer in the history of medicine at the Wellcome Institute for the History of Medicine, 183 Euston Road, London NW1 2BP, England. His publications include works on medicine and the Scottish Enlightenment, and nineteenth-century technology and medicine. The book *Photographic Medicine: Images and Power in Britain and America Since 1864* that he and Daniel Fox have collaborated on will appear in early 1988

JONATHAN M. LIEBENAU is Principal Research Fellow at the Technical Change Centre, 114 Cromwell Road, London SW7 4ES, England, U.K., a policy studies institute. His work focuses on social and economic studies of science, technology and medicine with particular reference to the finance and management of commercial technologies. His book, *Medical Science and Medical Industry The Formation of the American Pharmaceutical Industry*, will be published shortly by the Johns Hopkins University Press.

J. M. THIJSSEN holds degrees in philosophy and history and has recently completed his doctoral dissertation. He is interested in fourteenth-century natural philosophy. His address is Muzenplaats 121, 6525 JB Nijmegen, The Netherlands

ARLEEN TUCHMAN is an assistant professor of history and Andrew W Mellon Fellow at Vanderbilt University, Box 1652B, Nashville, Tennessee 37235 She is completing a study of the institutionalization of medicine at the University of Heidelberg, and plans next to examine the influence of military interests on medical education and medical care in eighteenth- and nineteenth-century Prussia

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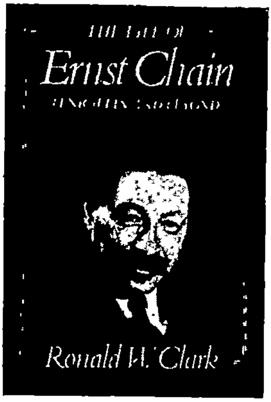

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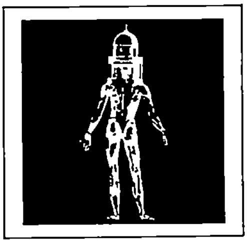

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